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PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
LONDON.

VOL. LXXVI. For the Year 1786.

PART I.



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A D V E R T I S E M E N T.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society ; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices ; which in some instances have been too lightly credited, to the dishonour of the Society.

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THE President and Council of the Royal Society adjudged,
for the Year 1785, the Medal on Sir GODFREY COPLEY'S
Donation, to Major-General WILLIAM ROY, for his Mea-
surement of a Base on Hounslow-Heath.

1. The first part of the paper is devoted to a
discussion of the various methods which have
been proposed for the determination of the
concentration of the various components of a
mixture.

PHILOSOPHICAL
TRANSACTIONS.

- I. *Observations on the Graduation of Astronomical Instruments; with an Explanation of the Method invented by the late Mr. Henry Hindley, of York, Clock-maker, to divide Circles into any given Number of Parts. By Mr. John Smeaton, F. R. S.; communicated by Henry Cavendish, Esq. F. R. S. and S. A.*

Read November 17, 1785.

PERHAPS no part of the science of Mechanics has been cultivated by the ingenious with more assiduity, or more deservedly so, than the art of dividing Circles for the purpose of Astronomy and Navigation. It is said, that TYCHO BRAHE
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and HEVELIUS laboured this part of their instruments with their own hands; and though public rewards have at length brought forth different methods of dividing from our best artists, which have been communicated to the Public; yet I trust it will be thought, that if any thing relative to this business remains yet behind, that may tend to furnish the ingenious artists, who are cultivating this field, with any new or curious idea upon the subject, it will be well worth communicating to this learned Society: since, if an hint, which is essentially different from any thing that (so far as I know) the Public is in possession of, be once started, and is pursued and worked upon by ingenious men, it is not possible to say, to what valuable purposes it may be converted.

This, perhaps, will better appear by taking a short review of the labours of others, from the time of TYCHO BRAHE and HEVELIUS (who did not use telescopic sights) to the present time.

The very learned, ingenious, and inventive Dr. HOOK, in his *Animadversions on the Machina Cœlestis* of HEVELIUS, published in the year 1674, has given us an elaborate description of a quadrant, whose divisions were formed, and afterwards read off, by means of an endless screw, working upon the outermost border of the limb of a quadrant; which, he says, *does not at all depend upon the care and diligence of the instrument-maker in dividing, graving, or numbering the divisions, for the same screw makes it from end to end*; yet he has given us no account of any particular care or caution that he used, in preventing the same screw from making larger or smaller paces, in consequence of unequal resistance, from a different hardness of the metal in different parts of the limb; nor any method of correcting or checking the same; nor of making a screw, the
 4 angle

angle of whose threads with the axis shall be equal in every part of the circumference; therefore the whole of this business (in which accurate *mechanists* well know consists the whole of the difficulty) he refers to the *ingenious workman*; and, in particular, to the then celebrated Mr. TOMPION, whom, he says, he employed to make his instrument, and who had thereby *seen and experienced the difficulties that do occur therein*: but was any ingenious workman now to pursue the directions of Dr. Hook, so far as his communication thereof extends, we may conclude, that he would make a very inaccurate piece of work, far inferior in performance to what the Doctor seems to expect from it*. But yet, I believe, it was the *first* attempt to apply the endless screw and wheel, or arch, to the purpose of forming divisions for astronomical instruments; for, the Doctor says himself, the perfection of this instrument is the *way* of making the divisions; that it *excels all the common ways of division*: and in the table of contents it is intituled, *An Explication of the new Way of dividing*.

This method, however, of Dr. Hook's was not laid aside without a very full and sufficient trial: for Mr. FLAMSTEED, in the *Prolegomena* of the third volume of *Historia Cælestis*, informs us, that *he* contrived the sextant, wherewith his observations were chiefly made, from his entrance into the Royal Observatory in the year 1676 to the year 1689. This sextant was first made of wood, and afterwards of iron, with a brass limb of two inches broad, by Mr. TOMPION, at the expence of Sir JONAS MOORE; the radius thereof was 6 feet $9\frac{1}{4}$ inches; it was furnished with an endless screw upon its limb of 17

* This was indeed verified in an attempt upon the same plan by the Duc DE CHAULNES, published in a Memoir of the Royal Academy of Sciences at Paris, for the year 1765.

threads in an inch, and with telescopic sights*. Of this instrument Mr. FLAMSTEED gives the figure at the latter end of his *Prolegomena* before-mentioned, sufficiently large to see the general design; the whole being mounted upon a strong *polar axis* of iron, of three inches diameter.

Though, in the full description of this instrument, Mr. FLAMSTEED mentioned the Limb's being furnished with *diagonal divisions*, distinguishing the arch to 10 seconds†; yet it is pretty clear, that it had not these originally upon it; but that the dependance was wholly upon the screw divisions, when it came out of Mr. TOMPION's hands. This one may reasonably infer from the observations themselves; for the first observation, set down as taken with this instrument, being upon the 29th of October, 1676, it was not till the 11th of September, 1677, that the column which contained the *check angle* by diagonal lines was filled up; and there was also a space of time, antecedent to that last mentioned, wherein no observations are recorded as taken with this instrument, in which time the diagonal divisions might be put on; and this will be put beyond a doubt, as he says expressly, that finding, in the year 1677, that the threads of the screw had worn the border of the limb, he divided the limb into degrees himself, and drew a set of diagonal divisions‡; and then comparing the two sets of divisions together, he sometimes found them to differ a whole minute; wherefore, for correction thereof, he constructed a new table for conversion of the revolutions and parts of the screw into degrees, minutes, and seconds; and

* “— Qualem nemo, cœlo adhibens;—” Preface to *Historia Cœlest.* printed in one vol. 1712.

† *Prolegomena Histor. Cœlest.* vol. III. p. 104.

‡ *Ibid.* p. 106. “Gradus in limbo distribui ac diagonales duxi.”

which

which he applied in the observations taken in 1678.—However, notwithstanding this correction, in looking over the observations noted down as deduced each way, I frequently find a difference of half a minute; not unfrequently 40''; but in an observation of the moon, of the 9th June, 1687, I find a difference of 55''*, which upon a radius of 6 feet 9 inches amounts to more than $\frac{1}{30}$ th part of an inch.

In the year 1689, Mr. FLAMSTEED completed his *mural arc* at GREENWICH; and, in the *Prolegomena* before-mentioned, he makes an ample acknowledgement of the particular assistance, care, and industry of Mr. ABRAHAM SHARP; whom, in the month of August, 1688, he brought into the observatory, as his *amanuensis*; and being, as Mr. FLAMSTEED tells us, not only a very skilful mathematician, but exceedingly expert in mechanical operations †, he was principally employed in the construction of the mural arc; which in the compass of fourteen months he finished, so greatly to the satisfaction of Mr. FLAMSTEED, that he speaks of him in the highest terms of praise ‡.

This celebrated Instrument, of which he also gives the figure at the end of the *Prolegomena*, was of the radius of 6 feet 7½ inches; and, in like manner as the sextant, was furnished both

* Vol. I. of Hist. Cœlest. p. 343.

† “Qui mechanices perquam expertus, pariter ac matheseos peritus.” *Prolegomena*, vol. III. p. 108.

‡ “SHARPEIUS servus meus fidelissimus, ac omnibus quidem dotibus & facultatibus erat imbutus, quæ ipsum tam subtili & difficili operi obeundo idoneum redderent.” *Prolegom. ibid.*

And on finishing the instrument, he says, “Gradus describuntur sive narrantur et exsculpantur, artificiosa manuali opera dicti domini SHARP, qui limbum partitus est, diagonales duxit, totumque organum absolvit et perfecit: adeo ut præstantissimi quivis artifices postquam illud conspexerunt et considerarunt, se exactius id peragere non potuisse, agnoverint.” *Prolegom. p. 111.*

with

with screw and diagonal divisions, all performed by the accurate hand of Mr. SHARP. But yet, whoever compares the different parts of the table for conversion of the revolutions and parts of the screw belonging to the mural arc into degrees minutes and seconds *, with each other, at the same distance from the zenith on different sides; and with their halves, quarters, &c. will find as notable a disagreement of the screw-work from the hand-divisions, as had appeared before in the work of Mr. TOMPION: and hence we may conclude, that the method of Dr. HOOK, being executed by two such masterly hands as TOMPION and SHARP, and found defective, is in reality not to be depended upon in nice matters.

From the account of Mr. FLAMSTEED it appears also, that Mr. SHARP obtained the zenith point of the instrument, or *line of collimation*, by observation of the zenith stars, with the face of the instrument on the east and on the west side of the wall †: and that having made the index stronger (to prevent flexure) than that of the sextant, and thereby heavier, he contrived, by means of pulleys and balancing weights, to relieve the hand that was to move it from a great part of its gravity ‡.

I have been the more particular relating to Mr. SHARP, in the business of constructing this mural arc; not only because we may suppose it the first good and valid instrument of the kind, but because I look upon Mr. SHARP to have been the first person that cut accurate and delicate divisions upon astronomical instruments; of which, independent of Mr. FLAMSTEED's testimony, there still remain considerable proofs: for, after leaving Mr. FLAMSTEED, and quitting the department above-mentioned †,

* Hist. Cœlest. vol. II. Appendix.

† Prolegom. p. 109.

‡ Mr. SHARP continued in strict correspondence with Mr. FLAMSTEED so long as he lived, as appeared by letters of Mr. FLAMSTEED's found after Mr. SHARP's death; many of which I have seen.

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he retired into *Yorkshire*, to the village of *Little Horton*, near *Bradford*, where he ended his days about the year 1743; and where I have seen not only a large and very fine collection of mechanical tools (the principal ones being made with his own hands), but also a great variety of scales and instruments made therewith, both in wood and brass, the divisions whereof were so exquisite, as would not discredit the first artists of the present times: and I believe there is now remaining a quadrant, of four or five feet radius, framed of wood, but the limb covered with a brass plate; the subdivisions being done by diagonals, the lines of which are as finely cut as those upon the quadrants at *Greenwich*. The delicacy of Mr. SHARP's hand will indeed permanently appear from the copper-plates in a quarto book, published in the year 1718, intituled, *Geometry improved by A. Sharp, Philomath.* whereof not only the geometrical lines upon the plates, but the whole of the engraving of letters and figures, were done by himself, as I was told by a person in the mathematical line, who very frequently attended Mr. SHARP in the latter part of his life. I therefore look upon Mr. SHARP as the first person that brought the affair of hand division to any degree of perfection.

Some time about the establishment of the mural arc at *Greenwich*, the celebrated Danish Astronomer OLAUS ROEMER began his domestic Observatory, which he finished in the year 1715, as we are informed by his historian PETER HORREBOW, in the third volume of his works, in the tract, intituled, *Basis Astronomiae*, published in the year 1741. In this tract is the description of an instrument, Tab. III. which not only answered the purpose of the meridian arc; but, its telescope being mounted upon a long axis, became also in reality what we now call a *Transit Instrument*; and which furnished, so far

as I have been able to learn, the first idea thereof. One end of the axis of this instrument being the center of the meridian arc, and carrying its index, M. ROEMER thereby avoided the errors arising from the plane of the mural arc not being accurately a vertical plane; and which Mr. FLAMSTEED endeavoured to check, by observing the passage of known stars nearly in the same parallel of declination; that is, passing nearly over the same part of the plane of the arc; by which he was enabled to correct or check the errors of the arc in right ascension. But it is the peculiar method in which ROEMER *divided* his instruments, that occasions him here to be introduced.

Though it is a very simple problem by which geometricians teach how to divide a given right line into any number of parts required; yet it is still a much more simple thing to set off upon a given right line, from a point given, any number of equal parts required, where the total length is not exactly limited; for this amounts to nothing more than assuming a convenient opening of the compasses, and beginning at the point given, to set off the opening of the compasses as many times in succession, as there are equal parts required; which process is as applicable to the arch of a circle as it is to a right line. Of this simple principle ROEMER endeavoured to avail himself.

For this purpose M. ROEMER took two stiff, but very fine-pointed, pieces of steel, and fixed them together, so as to avoid, as much as possible, every degree of spring that would necessarily attend long-legged compasses, or even those of the shortest and stiffest kind when the points are brought near together. The distance of the points that he chose was about the $\frac{1}{10}$ or $\frac{1}{12}$ of an inch. This, upon a radius of $2\frac{1}{2}$ or three feet, would be about 10 minutes. With this opening, beginning at

at the point given, he set off equal spaces in succession to the end of his arch, which was about 75° . Those were distinguished upon the limb of the instrument by very fine points, which were referred to by a grosser division, the whole being properly numbered. The subdivision of those arches of 10 minutes each was performed by a double microscope, carried upon the arm or radius of the instrument, the common focus being furnished with parallel threads of single silk, whereof eleven being disposed at ten equal intervals, comprehending together one ten-minute division, the distance of the nearest threads became a very visible space, answerable to one minute each, and therefore capable of a much further subdivision by estimation.

The divisions of this instrument were therefore, properly speaking, not degrees and minutes; but yet, if exactly equal, would serve the purpose as well, when their true value was found, which was done by comparison with larger instruments.

Now, if it be considered, that in going step by step of ten minutes each, through a space of 75 degrees, there will be a succession of 450 divisions, dependant upon each other; if it be also considered, that the least degree of extuberance in the surface of the metal, where each new point is set down, or the least hard particle (wherewith all the base metals seem to abound) will cause a deviation in the first impression of a taper point, and thereby produce an inequality in the division; it is evident, that though this inequality may be very small, and even imperceptible between neighbouring divisions, yet among distant ones, it may and will arise to something considerable; which, in the mensuration of angles, will have the same ill tendency as in near ones. Now, as M. ROEMER has given us no

means of checking the distant divisions, in respect of each other, it is very probable that no one has followed his steps, in cases where great accuracy was required, in a considerable number of divisions. For in reality this method is likely to fall far short of Dr. Hook's; as Dr. Hook's divisions being cut in a similar successive manner, by the rotation of the sharp *edge* of the threads of a screw against the exterior edge of the limb of the instrument, a very slight degree of pressure will bring a fine screw of thirty threads in an inch (which he prescribes) to touch against an arch whose radius is four or five feet in more than one, two, three, or four threads at once; so that the threads supporting one another, a small extuberance, or even a small hard particle in the metal, will be cut through or removed by the grinding or rather sawing motion of the screw; and which, in regard to its contact, being in reality an edge, will be much more effectual (that is, more firm) in its retention than a mere simple point: and a repetition of the operation, from the support of the threads to each other, will tend to mend the first traces; whereas, in ROEMER's way, a repetition will make them worse; for, whatever drove forward or backward the point on first entering, will, from the sloping of the point, be confirmed and increased in driving it deeper.

When Dr. HALLEY was chosen Astronomer Royal (Mr. FLAMSTEED's instruments being taken away by his executors), Mr. GRAHAM undertook to make a new mural quadrant, about the year 1725; who, uniting all that appeared valuable in the different methods of his predecessors, executed it with a degree of contrivance, accuracy, and precision, before unknown: and the division thereof he performed with his own hand. The model of this quadrant, for strength, easy management, and convenience, has been ever since pursued as the most perfect.

What

What I apprehend to be peculiar in it, was the application of the arch of 96° ; not only as a check upon the arc of degrees and minutes, but as superior thereto, being derived from the more simple principle of *continual bisection*.

To make room for this, he has entirely rejected the subdivision by diagonals, and has adopted the method of the *Vernier*; but the subdivision of the vernier divisions he, as I apprehend for the first time, measured by the turns of the detached adjusting screw, making it in fact a micrometer, by which the distance of the *set* of the instrument was to be measured from the perfect coincidence of one of the actual divisions of the limb with the next stroke of the vernier; by which means the observation could not only be read off with all the precision that the division of the instrument was capable of, but the two sets of divisions could be checked and compared with each other. Another thing that I apprehend to be peculiar in this instrument, was the more certain method of transferring and cutting the divisions, from the original divided points, by means of the *beam-compass*, than could possibly be done from a *fiducial edge*, as had doubtless been constantly the practice in cutting diagonals; for, placing the steady point of the beam-compass in the tangent line to that part of the arc where each division was to be cut, the opening of the compass being nearly the length of the tangent, the other point would cut the division in the direction of the radius nearly; and though in reality an arch of a circle, yet the small part of it in use would be so nearly a right line, as perfectly to answer the same end; all which advantages put together, it is probable, induced Mr. GRAHAM to reject the diagonals.

Soon after the completion of this quadrant, Mr. GRAHAM undertook to execute a *zenith sector* for the Rev. Dr. BRADLEY,

which was fixed up at *Wanstead*, in *Essex*, in the year 1727. The very simple construction that he adopted for this instrument (the plumb line itself being the index) did not admit of the use of a vernier: he therefore contented himself with dividing the arch of the limb of this instrument by primary points, as close as he thought necessary, that is, by divisions of five minutes each, and measuring the distance from the *set* of the instrument to the next point of division by a *micrometer* screw, in the construction of which screw he used uncommon care and delicacy. I have mentioned this instrument to introduce this observation; that I think it highly probable, had Mr. GRAHAM constructed the great quadrant *after* the zenith sector had been fully tried, he would have rejected not only the diagonals but the verniers also, as containing a source of error within themselves which may be avoided by a well-made screw*.

It seems also, that Mr. GRAHAM, at the time he constructed both these instruments, was not aware how much error could arise from the unequal expansions of different metals by heat or cold: for in both the radius, or frame of the instrument, was iron, while the limbs were of brass. They, however, remain in the Royal Observatory, perfect models, in all other respects, of every thing that is likely to be attained in their respective destinations, and monuments of the superlative abilities of that great mechanician Mr. GRAHAM†.

* This has been found consentaneous to the experience of my friend Mr. ANBERT, who, on my suggestion, has long since laid aside the use of his vernier, measuring always by the micrometer screws the distance between the set of the instrument, and the coincidence of the first stroke of the vernier with the next primary division of the limb.

† I have been informed, that Dr. MASKELYNE has caused this objection to the sector to be rectified, since its removal to the Royal Observatory, by substituting an iron limb instead of that of brass, the points being made upon studs of gold.

Mr.

Mr. GRAHAM lived till the year 1751 ; and during his time there were few instruments of consequence constructed without his advice and opinion. They were for many years done by Mr. Sisson, to whom doubtless Mr. GRAHAM would fully communicate his method of division ; and from this school arose that very eminent and accurate artist Mr. BIRD, whose delicate hand, joined with great care and assiduity, enabled him still further to promote this branch of division ; and which being carried by him to a great pitch of perfection, the Commissioners of Longitude did themselves the credit, by an handsome reward, to induce him to publish to the world his particular method of dividing astronomical instruments ; which being drawn up by himself, in the year 1767, this matter is fully set forth to the public : I shall therefore only take this opportunity of observing, that there seems to be one article in which Mr. BIRD's method may be still improved.

I must here observe, that I apprehend no quadrant, that has ever undergone a severe examination, has been found to form a perfect arch of 90° ; nor is it at all necessary it should : the perfect equality of the divisions throughout the whole is the first and primary consideration ; as the proportion of error, when ascertained by proper observations, can be as easily and readily applied, when the whole error of the rectangle is fifteen seconds, as when it is but five.

In this view, from the radius taken, I would compute the chord of sixteen degrees only. If I had an excellent plain scale, I would use it ; because I should expect the deviation from the right angle to be less than if taken from a scale of more moderate accuracy ; but if not, the equality of the divisions would not be affected, though taken from any common diagonal scale. This chord, so prepared, I would lay off five times in succession, from
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the primary point of 0 given, which would compleat eighty degrees; I would then bisect each of those arches of 16° , as prescribed by Mr. BIRD, and laying off one of them beyond the 80th, would give the 88th degree: proceeding then by bisection, till I came to an arch of two degrees, laying that off from the 88th degree, would give the point of ninety degrees. Proceeding still by bisection, till I had reduced the degrees into quarters = fifteen minutes each, I would there stop; as from experience I know, that when divisions are over close, the accuracy of them, even by bisection, cannot be so well attained as where they are moderately large. If a space of $\frac{1}{16}$ of an inch, which is a quarter of a degree, upon an eight-foot radius, is thought too large an interval to draw the index over by the micrometer screw, this may be shortened by placing another line at the distance of one-third of a division on each side of the index line, in which case the screw will never have to move the index plate more than one-third of a division, or five minutes; and the perfect equality of those side lines from the index line may be obtained, and adjusted to five minutes precisely, by putting each of the side lines upon a little plate, capable of adjustment to its true distance from the middle one, by an adjusting screw.

The above hint is not confined to the chord of sixteen degrees, which prohibits the subdivisions going lower than fifteen minutes: for if it be required to have divisions equivalent to five minutes upon the limb itself; then I would compute the chord of $21^{\circ} 20'$ only; and laying it off four times from the primary point, the last would mark out the division $85^{\circ} 20'$, pointed out by Mr. BIRD; *supplying the remainder* to a quadrant, from the bisected divisions as they arise, and not by the application of other computed chords.

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In my Introduction to M. ROEMER's Method of Division, I have shewn, that divisions laid off in succession, by the same opening of the compasses, either in a right line, or in the arch of a circle, being in its idea geometrically true, and in itself the most simple of all processes, it has the fairest chance of being mechanically and practically exact, when cleared of the disturbing causes. The objection therefore to his method is, the great number of repetitions, which depending upon each other in succession (requiring no less than 540 to a quadrant, when subdivided to ten minutes each), the smallest error in each, repeated 540 times, without any thing to check it by the way, may arise to a very sensible and large amount: but in the method I have hinted, this objection will not lie; for, in the first case, the assumed opening is laid off but five times; and in the latter case but four times; nor does this *repetition* arise out of the nature of the thing; for, if you like it better, you may, in the former case, at once compute the chord of 64° ; and in the latter that of $85^\circ 20'$, and then proceed wholly by bisection; supplying what is wanted to make up the quadrant, from the bisected divisions, as they arise. Mr. BIRD prescribes this method himself, for the division of HADLEY's sextants and octants.

He, I suppose, was the first, who conceived the idea of laying off chords of arches, whose subdivisions should be come at by continual bisection; but why he mixed therewith divisions that were derived from a different origin (as prescribed in his method of dividing) I do not easily conceive. He says, that after he had proceeded by the bisections, from the arc of $85^\circ 20'$, the several points of 30° , 60° , 75° , and 90° , (all of which were laid down from the principle of the chord of 60° being equal to radius), *fell in without sensible inequality*; and so indeed they might; but yet it does not follow that they were equally true in their places.

as

as if they had been (like the rest) laid down from the bisection from $85^{\circ} 20'$, and therefore being the first made, whatever error was in them, would be communicated to all connected with them, or taking their departure from them. Every heterogeneous mixture should be avoided where equal divisions are required. It is not the same thing (as every good artist will see) whether you *twice* take a measure from a scale as *nearly* the same as you can, and lay them off separately; or lay off *two* openings of the compasses, in succession, *unaltered*; for though the same opening, carefully taken off from the same scale a second time, will doubtless fall into the points made by the first, without sensible error; yet as the sloping sides of the conical cavities made by the first point will conduct the points themselves to the center, there may be an error which, though insensible to the sight, would have been avoided by the *more simple process* of laying off the opening twice, without ever altering the compasses.

The 96 arc was I have no doubt, invented by Mr. GRAHAM, from having perceived, in common with all preceding artists, how very much more easy a given line was to bisect, than to trisect, or quinquesection; and therefore the 96 arc which proceeded by bisections only (or by laying off the same identical openings, which, as already shewn, is still more simple and unexceptionable) was wholly intended by him by way of checking the division of the arc of 90, which required trisections and quinquesections. But experience soon shewed the superior advantage of it so strongly, that the use of the 90 arc is now wholly set aside, where accuracy is required; whereas the ingenuity of Mr. BIRD having shewn a way to produce the 90 arc by bisection, when this is really pursued quite through the piece, by rejecting all divisions derived from any other origin, the 90 arc will have nothing in it to prevent its being equally

equally unexceptionable with the 96 arc; and consequently if, instead of the 96 arc, another arc of 90 was laid down (which being upon a different radius, its divisions will stand totally unconnected with the former), then these two arcs would in *reality* be a check upon each other; for being of equal validity, the mean might be taken: and if (in lieu of vernier divisions) strokes at the distance of any odd number, as 7, 9, 11, or 13, are marked upon, and carried along with, the index plate; these will produce a check upon neighbouring divisions; and the angle may then be deduced from the medium of no less than four readings.

The last works that have been made known to the public in the line of graduation (so far as has come to my knowledge) are those of the very ingenious Mr. RAMSDEN, which were published, by order of the Board of Longitude, in the year 1777.

From his own information, I learn, that in the year 1760 he turned his thoughts towards making an engine for dividing mathematical instruments; and this he did in consequence of a reward offered by the Board of Longitude to Mr. BIRD, for publishing his method of graduating quadrants; for as, several years previous to that period, he had taken great pains to accomplish himself in the art of hand-dividing, in which line Mr. BIRD had acquired his eminence, he conceived by this publication of Mr. BIRD's he should be reduced to the same standard of performance with the rest of the trade. He, therefore, partly to save time, and that kind of weariness to an ingenious mind that ever must attend the endless repetition of the same thing from morning to night; partly still to preserve the pre-eminence he had then gained; and partly to procure dispatch in the great increase of demand for HADLEY's sextants and octants, in consequence of the successful application of the moon's motion to the

purpose of ascertaining the longitude at sea (which instruments for this purpose required a degree of accuracy and certainty in the division, by no means necessary thereto when applied to the simple purpose of observing latitudes); I say, for these considerations, Mr. RAMSDEN determined to set about something in the instrumental way, that should be sufficient effectually to answer these purposes.

Accordingly, considering the nature of the endless screw, he set himself to work upon an engine whose divided wheel or plate was of thirty inches diameter; and though the performance of this first essay was inferior to his expectations and wishes, yet with it he was able to divide theodolites with a degree of precision far superior to any thing of the kind that had been exhibited to the public.

This engine I myself saw in the spring of the year 1768; and it appeared to me not only a very laudable attempt towards instrumental divisions, but a very good model for the construction of an engine of the most accurate kind for that purpose. And he furthermore, at the same time, shewed me the model or pattern for casting a wheel of a much larger size, which he proposed to make upon the same plan, and with considerable improvements. This being effected some time in or about the year 1774, its accuracy was proved by making a sextant, afterwards subjected to the examination of Mr. BIRD; who in consequence approved the method, not only as fully sufficient for the division of HADLEY's sextants and octants for any purpose whatsoever, but in fact for dividing any instrument whose radius did not exceed that of the dividing wheel, which was forty-five inches in diameter: whereupon the Board of Longitude, ever ready to encourage all endeavours that tend to the certainty and perfection of any thing subservient to the purpose of finding the longitude at sea, very properly and usefully resolved to confer

an handsome reward on Mr. RAMSDEN, for delivering a full explanation of his method of making the said engine; which, in consequence, was published by order of the Board of Longitude in the year already mentioned, viz. 1777: the designs whereof are so full and explicit, that whoever could not understand that description, so as to enable him to make it, would be unfit to undertake it on other accounts.

From what I have said upon the works of the different artists that I have mentioned, it would seem that the art of graduation was brought to that degree of perfection, that nothing material can now be added thereto: and I should have been apt to have thought so myself, if I had not happened, in the course of my life, to have had a communication made to me (under the seal of secrecy) which seems to promise yet further light and assistance in perfecting that important art; and every impediment to the discovery thereof being now removed, I shall in the remainder of this essay give the clearest description thereof that I am able, with such elucidations and improvements as seem to be naturally pointed out by the method itself.

In the autumn of the year 1741, I was first introduced to the acquaintance of that then eminent artist, Mr. HENRY HINDLEY, of *York*, clockmaker;—he immediately entered with me into the greatest freedom of communication, which founded a friendship that lasted to his death, which did not happen till the year 1771, at the age of 70.. On the first interview, he shewed me not only his general set of tools, but his *engine*, at that time furnished with a dividing plate, with a great variety of numbers for cutting the teeth of clock wheels, &c. and also, for more nice and curious purposes, furnished with a wheel of about thirteen inches diameter, very stout and strong, and cut into 360 teeth; to which was applied an endless screw, adapted thereto. The

threads of this screw were not formed upon a cylindric surface, but upon a solid whose sides were terminated by arches of circles. The whole length contained fifteen threads; and as every thread (on the side next the wheel) pointed towards the center thereof, the whole fifteen were in contact together; and had been so ground with the wheel, that, to my great astonishment, I found the screw would turn round with the utmost freedom, interlocked with the teeth of the wheel, and would draw the wheel round without any shake or sticking, or the least sensation of inequality.

How long this engine might have been made before this first interview, I cannot now exactly ascertain: I believe not more than about a couple of years; but this I well remember, that he then shewed me an instrument intended for astronomical purposes, which must have been produced from the engine, and which of itself must have taken some time in making*.

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* This instrument was of the equatorial kind; the wheel parallel to the equator, the quadrant of latitude, and semi-circle of declination, being all furnished with screws containing fifteen threads each, framed and moved in the same manner as that of the engine; the whole of which instrument was already framed, and the telescope tube in its place, which was intended to be of the inverting refracting kind, and to be furnished with a micrometer. This, however, was not completed till some years after; but, in the year 1748, I received it in London for sale. It staid with me two years, in which time I shewed it to all my mechanical and philosophical friends, amongst whom was Mr. SHORT, who afterwards published in the Philosophical Transactions, vol. XLVI. N^o 493. p. 241. an Account of a portable Observatory, but without claiming any particular merit from the contrivance. However, the model of it differs from HINDLEY's equatorial only in the following articles. He added an azimuth circle and compass at the bottom. He omitted the endless screws, placing verniers in their stead; and at the top, a reflecting telescope instead of a refractor. This instrument of HINDLEY's being afterwards returned to him unfold, I pointed out the principal deficiencies that I found therein; viz. that, in putting the instrument into different positions,

I in reality thought myself much indebted to Mr. HINDLEY for this communication ; but though he shewed me his engine, and told me, that the screw was cut by the rotation of the point of a tool, carried round upon a strong arm, at the distance of the radius of the wheel from the center of motion, which arm was carried forward by the wheel itself, and the wheel was put forward by an endless screw, formed upon a cylinder to a proper size of thread, cut by his chock lathe ; though he shewed me also this chock lathe, and the method employed to make the threads of the screw *equiangular* with the axis, that is, to free the screw from what workmen term *drunkenness* ; and also shewed me how, by the single screw of his lathe, he could cut, by means of wheel-work, screws of every necessary degree of fineness * (and, by taking out a wheel, could cut a left-handed screw of the very same degree of fineness) ; by which means he was enabled not only to adapt his plain screw to the size of the teeth of his wheel, but also to prevent any drunkenness that otherwise the curved screw would be subject to in consequence of being produced from the plain

tions, the springing of the materials was such as in some positions to amount to considerable errors. This remained with him in the same state till the year of the first *Transit of Venus*, viz. 1761 ; when it was sold to ——— CONSTABLE, Esq. of *Barton Constable*, in *Holderness*. Mr. HINDLEY, to remedy the evil above-mentioned, applied balances to the different movements. He soon afterwards completed one, *de novo*, upon this improved plan, for his Grace the late Duke of NORFOLK. A method of balancing in much the same way, without the knowledge that it had been done before, has been fully explained, and laid before the Society, by our ingenious and worthy brother Mr. NAIRNE. *Phil. Trans.* vol. LXI. p. 108.

* A machine for cutting the endless screw of Mr. RAMSDEN's engine, upon principles exactly similar, is fully and accurately set forth in his Description of his dividing Engine above-mentioned.

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one; furthermore, that the screw and wheel, being ground together as an optic glass to its tool, produced that degree of smoothness in its motion that I observed; and, lastly, that the wheel was cut from the dividing plate: yet, how the dividing plate was produced, he for particular reasons reserved to himself.

Nor can he be blamed for the reservation of this one secret; as he had, even at the time of my early acquaintance with him, a kind of foresight that from the superior merit of HADLEY's quadrant, a demand for that, and other instruments for the purpose of navigation, was likely to increase; and that he might live to see a public reward offered for a method of dividing them with greater accuracy and dispatch than had at that time appeared. Indeed, he had himself an idea, from the satisfactory success that had attended his operations in dividing, that a screw and wheel, produced from his engines of one foot diameter, would have as much truth as the eight-feet quadrant at Greenwich: and though he doubtless greatly over-rated the accuracy of these miniature performances, yet it does not follow, as his methods were not confined to so narrow a compass, but that, his scale of operation being proportionably enlarged, a degree of accuracy in the graduation of astronomical instruments may be attained in proportion.

I must here beg leave to observe, that there appears to me to be a natural limitation to the accuracy of instruments, consisting of considerable portions of a circle, such as quadrants, &c. *. I do not find that the finest stroke upon the limb of a quadrant, made by BIRD's own hand, if removed from its

* The zenith sector consists but of few degrees, with little variation of its position in using it.

coincidence with its index, can be replaced with any degree of certainty nearer than the 4000th part of an inch, though aided by a magnifying glass *.

A 4000th part of an inch being then determined to be the *minimum visibile* by the strokes of an instrument, this will be less than one second of a degree upon a radius of four feet; and therefore, if the whole set of divisions upon the limb could be preserved true to this aliquot part of an inch, the eight-feet quadrants of Greenwich might be expected to be true to half a second. How far they are from this, I do not exactly know; but I have reason to think they vary from it some seconds: nay, I believe it is generally allowed, that our largest quadrants, even when executed by the accurate hand of Mr. Bird, do not exceed those of a less size, by the same hand, in proportion to their increase of radius: nor can it well be expected that they should; since, as the weight necessarily increases in a triplicate ratio of the radius, the great weight of the Greenwich quadrants in moving and fixing them (as they could not be divided in their place) may easily derange the framing; or even the *internal elasticity* of the materials may give way, by a change of position, to so minute a quantity as a 4000th part of an inch. It therefore appears to me, that since the divisions of a quadrant of four-feet radius are more than sufficient, and even those of three feet admit of all the distinctness that in other respects is wanted, a three-feet quadrant, in point of

* It will be to little purpose to attempt it with a greater power. Double-microscopes can doubtless be formed to magnify objects, far less than a 4000th part of an inch, to distinct surfaces; but then the advantage of such degrees of magnifying power is chiefly upon the organized bodies of nature. Let a dot, or the finest point that can be made by human art, be so viewed, and it will appear not round, but a very ragged irregular figure.

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size, is capable of all attainable exactness; and would be as much to be depended on as any of those now in being of eight feet. By adopting quadrants of this smaller size, we shall of course get rid of $\frac{1}{3}$ ths of the present weight; and consequently of much cumber, unhandiness, and derangement, that must arise from that weight, as well as the fear of totally discomposing them, if ever moved out of their place.

It now comes to be time to open a principle upon which there is a prospect of effecting such an improvement. I have shewn that a 4000th part of an inch is the *ultimatum* that we are to expect from *sight*, though aided by glasses, when observing the divisions of an instrument. But in the XLVIIth volume of the *Philosophical Transactions* for the year 1754, I have shewn the mechanism of a new *pyrometer*, and experiments made therewith; whereby it appears, that, upon the principle of *contact*, a 24,000th part of an inch is a very definite quantity. I remembered very well that I did not then go to the extent of what I might have asserted, being willing to keep within the bounds of *credibility*: but on occasion of the present subject, I have re-examined this instrument, and find myself very well authorised to say, that a 60,000th part of an inch, with such an instrument, is a more definite and certain quantity than a 4000th part of an inch is to the *sight*, conditioned as above specified. The certainty of contact is, therefore, fifteen times greater than that of vision, when applied to the divisions of an instrument: and if this principle of certainty in contact did not take place even much beyond the limit I have now assigned, we never should have seen those exquisite mirrors for reflecting telescopes, that have already been produced.

These reflections apply immediately to my present subject, as HINDLEY'S method of division proceeds *wholly by contact*, and that

that of the firmest kind; there being scarcely need of magnifying glasses in any part of the operation.

In the year 1748 I came to settle in London; and the first employment I met with was that of making philosophical instruments and apparatus. In this situation, my friend HINDLEY, from a principle the reverse of jealousy, fully communicated to me, by letter, his method of division; and though I was enjoined secrecy respecting others (for the reasons already mentioned), yet the communication was expressly made with an intention that I might apply it to my own purposes.

The following are extracts from two letters, which contain the whole of what related to this subject; and since I have many things to observe thereon, so that the paraphrase would be much greater than the text, I think it best not to interrupt the description with any commentary, as perhaps his own mode of expression will more briefly and happily convey the general idea of the work than any I can use instead of it.

MY DEAR FRIEND,

York, 14 Nov. 1748.

AS to what you was mentioning about my brother's knowing how I divided my engine plate, I will describe it as well as I can myself; but you will want a good many things to go through with it.

The manner is this: first chuse the largest number you want, and then chuse a long plate of thin brass; mine was about one inch in breadth, and eight feet in length, which I bent like an hoop for an hoghead, and foldered the ends together; and turned it of equal thickness, upon a block of smooth-grained wood, upon my great lath in the air (that is, upon the end of the mandrel): one side of the hoop must be rather wider than

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the other, that it may fit the better to the block, which will be a short piece of a cone of a large diameter: when the hoop was turned, I took it off, cut, and opened it straight again.



The next step was to have a piece of steel bended into the form as *per margin* *; which had two small holes bored in it, of equal bigness, one to receive a small pin, and the other a drill of equal size. I ground the holes after they were hardened, to make them round and smooth. The chaps formed by this steel plate were as near together as just to let the long plate through. Being open at one end, the chaps so formed would spring a little, and would press the long plate close, by setting in the *vise*. Then I put the long plate to a right angle to the length of the steel chaps, and bored one hole through the long plate, into which I put the small pin; then bored through the other hole; and by moving the steel chaps a hole forward, and putting in the pin in the last hole, I proceeded till I had divided the whole length of the plate.

The next thing was to make this into a circle again. After the plate was cut off at the end of the intended number, I then proceeded to join the ends, which I did thus: I bored two narrow short brass plates † as I did the long one, and put one on the inside, and the other on the outside of the hoop, whose ends were brought together; and put two or three turned screw pins, with flat heads and nuts to them, into each end, which held them together till I rivetted two little plates, one on each side of the narrow plate, on the outside of the hoop. Then I took out the screws, and turned my block down, till the hoop

* The figure is considerably less than the real tool should be.

† These I shall hereafter distinguish by the name of *saddle-plates*.

would fit close on ; and by that means my right line was made into an equal divided circle of what number I pleased.

The engine plate was fixed on the face of the block, with a steel hole fixed before it, to bore through ; and I had a point that would fall into the holes of the divided hoop ; so by cutting shorter, and turning the block less, I got all the numbers on my plate.

I need not tell you, that you get as many prime numbers as you please ; nor that the distance of the holes in the steel chaps must be proportioned to the length of the hoop.

You may ask my brother what he knows about my method of dividing ; but need not tell him what I have said about it ; for I think neither *he* nor *John Smith* know so much as I have *told you*, though I believe they got some knowledge of it in general terms *.——I desire you to keep the method of dividing *to yourself*, and conclude with my best wishes,

and am, dear Sir, yours, &c.

HEN. HINDLEY.

Though the above letter was in itself very clear and explicit, as to the general traces of the method, yet some doubts occurring to me, a further explanation became necessary. A copy of my letter not being preserved, the purport of it may be inferred from the answer, which was as follows :

* The persons here referred to were both bred with him. His brother, Mr. ROGER HINDLEY, who has many years followed the ingenious profession of a watch-maker in London, was so much younger as to be an apprentice to him. Mr. JOHN SMITH, now dead, had some years past the honour to work in the instrument way, under the direction of the late Dr. DEMAINEBRAY, for his present MAJESTY.

DEAR FRIEND,

York, 13 March, 1748-9.

I THINK in your last you seem to be apprehensive of some difficulties in drilling the hoop for dividing: First, that the center of the hole in the hoop might not be precisely in the center of the hole of the steel chaps, it was drilled in; but if I described fully to you the method I used, I can see no danger of error there: for my chaps were very thick, and the two corresponding holes were a little conical, and ground with a steel pin; first one pair, and then the other, alternately, till the pin would go the same depth into each. Then for drilling the hoop, I took any common drill that would pass through, and bore the hole. After that I took a five-sided broach, which opened the hole in the brass betwixt the steel chaps, but would not touch the steel; so consequently the center of the holes in the brass must be concentric with the holes in the chaps: and for alterations by air, heat, cold, &c. I was not above two or three hours in drilling a row of holes, as far as I remember.

2dly, For drilling, in a right line, I had a thin brass plate, fastened between the steel chaps, for the edge of the hoop to bear against, whilst I thrust it forward from hole to hole. What you propose of an iron frame with a lead outside, will be better than my wooden block; but considering the little time that pass, betwixt transferring the divisions of the hoop to the divisions of my dividing plate, I did not suffer much that way. It was when I drilled the holes in my dividing plate that I used a frame for drilling, which had one part of it that had a steel hole, that in lying upon the plane of the dividing plate was fixed fast in its place for the point of the drill to pass through: then, at the length of the drill, there was another piece of steel,

steel, with a hole in it, to receive the other end of the drill to keep it at right-angles to the plane of the plate. This piece was a spring, which bended at the end, where it was fastened to the frame of the lathe, at about eighteen inches from the end of the drill; so it pushed the drill through with any given force the drill would bear: and though that end of the drill moved in the arch of a circle, it was a very small part of it, being no more than equal to the thickness of the dividing plate.

My good wishes conclude me yours,

HEN. HINDLEY.

Whoever attentively considers the communication contained in the above letters will see, that more happy expedients could not have been devised to procure a set of divisions, where there should be the most exact equality among *neighbours*; and which, for the purposes of clock-making, is the principal thing to be wished for. But herein, as in M. ROEMER's method, there were no means of checking the distant divisions, which run on to 360: now such a check, when the expansion of metals is considered, and particularly the difference of expansion between brass and steel, seems absolutely necessary for the purpose of divisions upon instruments, where the accurate mensuration of large angles is required, as much as the equality of neighbouring divisions*.

With this view the invention of this ingenious person suggested to him the thought of making his curved screw to lay

* The ingenious Mr. STANCLIFFE (some years a workman of HINDLEY's) has suggested, that the difference of expansion between the steel chaps and the brass hoop may be avoided by making the chaps of brass also, with hard steel holes set separately therein, somewhat similar to the jewelled holes of watches.

hold of fifteen teeth or degrees together : this, in effect, becomes a pair of compasses, 24 removes of which complete the whole circle, and produce 24 checks in the circumference : and whoever considers the very exquisite degree of truth that results from the grinding of surfaces in contact, as already noticed, must expect a very great degree of rectification of whatever errors might subsist in the wheel after its first cutting.

What degree of truth it might in reality be capable of upon its first production and adjustment, is not now to be ascertained, he never having used it for the graduation of any capital instrument. Those that he made with a view to an accurate measure of angles, he always made with a screw and wheel, or parts of circles cut by his engine into teeth, and ground together as before-mentioned ; but I have reason to think, that its performance, if put to a strict test, was never capable of that accuracy that he himself supposed it to have.

The method itself, however, from its simplicity, and ease of execution, seems to me to be a foundation for every thing that can be expected in truth of graduation ; and in consequence for reducing instruments to the least size that is capable of bringing out all that can be expected from the largest ; when it shall, like manual division, have received those advantages that the joint labours of the most ingenious men can bestow upon it. That I may not appear to be without grounds for my expectations, I will beg leave to propose, what near forty years occasional contemplation has suggested to me on the subject ; and as I can describe the process I would pursue, where different from HINDLEY's, in fewer words than I could make out a regular criticism upon his letters, I will immediately proceed to the description of it.

Proposed Improvements of HINDLEY's Method.

I would recommend the number of parts into which the circle is to be reduced to be 1440; that is 4 times 360; which divisions will therefore be quarters of a degree; the distances of the holes in the chaps will therefore, to a three-feet radius, be $\frac{1}{16}$ of an inch nearly; that is, between the one-sixth and one-seventh of an inch distance center and center.

Having provided myself with a stout mandrel, or arbor, for a *chock Lathe*, properly framed, that would turn a circle of six feet diameter, I would prepare a chock, or platform, for the end of it, of that diameter, or a little more, composed of clean-grained mahogany plank, all cut out of the same log; which, when finished, to be about $1\frac{1}{2}$ inch thick, and formed in sectors of circles, suppose 16 to make the circle; the middle line of each sector lying in the direction of the grain of the wood, this will consequently every where point outward: the method of framing this kind of work is well known.

The way of getting a slip of brads to answer the circumference of this platform is suggested in Mr. BIRD'S Account of constructing Mural Quadrants. Let a parallelogram of brads of about three feet long, and of a competent substance (suppose half an inch) to make it when finished about one-twentieth of an inch in thickness, be cast of the finest brads; and this to be rolled down till it becomes of sufficient length for the hoop, and about one-fifth part more. I would then cut off, from the whole length, somewhat better than one-sixth part, the whole being sufficiently reduced to a thickness by the rollers. Perhaps no way will be more ready and convenient to get

get such a long strip of brafs reduced to an equal breadth, than the method prefcribed by HINDLEY; viz. by turning it upon the chock prepared; but I would not make it wider on one fide than the other, like the hoop of a cask, as he describes, but exactly to fit the chock, when truly cylindric; for the internal elasticity of the brafs, in fo great a length, will be very fufficient for fitting it on tight enough, without any tapering. This I will now fuppose done; and a pair of steel *chaps*, as described by HINDLEY, to be alfo prepared, and ready for grinding; which, by fuch a careful admeafurement as can eafily be made, will give the length of the hoop fufficiently near, on its firft preparation.

Method of forming a Pair of Straps as a check to the Divisions.

The part firft cut off muft be again cut into two equal parts in length; which, for diftinction fake, I will call the *straps*; and which are to ferve as checks to every 60th and every 120th divifion of the circle.

A fteel plate, of about half an inch in breadth, the fame thicknefs as the straps, and in length equal to the breadth of the hoop plate, muft be foldered with filver folder to one end of each of the straps, by which means their length will be increafed half an inch by the fteel. An hole muft then be made through each fteel plate, of the fame fize as thofe through the chaps, and anfwerable to the middle of the straps; but fo near the border of the fteel, that when the chaps are put on, and adapted to the fteel hole, the next hole will fall through the brafs. The fteel plates muft then be hardened; and a pin being put through the two holes and the two plates, thefe muft be

be wrought to a right line in contiguity to each other; by this means the straight edge of each of the straps will be reduced to the same distance from the steel hole: the hard steel edges may be rectified by the grindstone, if necessary.

This being done, not only the holes in the chaps, but the holes in the two steel plates, applied to each other, like the two sides of the chaps, must be respectively ground together; not with a taper pin, as prescribed by HINDLEY; but so as not only to be cylindrical, but that the same cylindrical pin shall equally fit them all, and leave them smooth and polished; which is a process no ways difficult to a curious artist, and of which therefore a minute description is unnecessary.

The chaps being then put upon one of the straps, with its straight edge uppermost, and a pin put through the holes on the left-hand, and through the steel hole in the strap under operation, the chaps must be set upright, so that the line joining the centers of the holes shall be parallel to the upper edge of the strap; the brass plate, mentioned by HINDLEY, between the chaps, as a guide for directing them always to that upright position, may be then adjusted and fixed to the inside of the chap next the operator*.

The performance of the ensuing part of this work should be at a season when the temper of the air is not very variable; rather above the mean temper (suppose at 60°) than below it;

* It would be well, previous to the drilling of the steel chaps, that another hole was drilled in the chaps, that should be somewhat above the upper edge of the straps, and in the middle betwixt side and side, to receive a *steady pin* therein, antecedent to drilling the main holes; for then a tempered steel pin, a little taper, will, by driving it in as far as necessary, constantly answer this purpose from first to last, so as to regulate the holes in grinding, to be truly opposite: proper holes should also be drilled for fixing the brass guide plate to one of the chaps.

but above all things the artist should be himself cool ; that is, not in a state of sensible perspiration ; and there should be a free circulation of air in the room. Things being thus conditioned in respect to temperature, he may begin to drill the holes in one of the straps ; the pin being first put through the chaps and through the steel hole of the strap ; and the next hole, being drilled through the brass with a common drill, that and every hole as it goes is to be finished with a taper broach, as prescribed by HINDLEY ; and he may then prove or finish every hole by the application of a thorough broach, made so full as to require a degree of pressure to force it through ; and this broach being a little tempered, and the holes quite hard, there will be no fear of injuring the steel holes*.

Calling the hole in the steel plates o, and observing the time of beginning, you may proceed to drill 60 holes as prescribed by HINDLEY ; and noting how long you have been about it, you may lay the work aside a length of time, equal to the time you took in drilling ; that any addition of warmth it may have acquired in handling or working may be again lost in a great degree †. After this pause you may begin again, and go on to finish 60 holes more ; that is, to the length of

* The steel holes in the chaps need not to be above one-twentieth of an inch in diameter ; and though it may be proper to make the steel plate, of which they are formed, one-tenth of an inch thick, in order to give the spring formed between them a convenient degree of stiffness, yet they may be reduced (by chamfering the outsides) to half that thickness.

† As there is not much occasion for the artist to touch his work, the effects of that may also be very much avoided by wearing thick gloves ; and the friction being but slight, and the work almost continually in the vise, the variation of temperature in the metals concerned cannot be sensible or considerable.

120 holes from the beginning; you then proceed in the same manner with the other strap.

Method of drilling the Hoop.

You are now prepared to commence the work upon the long or *hoop-plate*; and you proceed therewith, in forming the first hole with the chaps, as before directed by HINDLEY, and this first hole you call 0. You then place the straps one on each side the hoop, with their gaged edges upward, and put the pin through the holes denominated 60 upon the straps, and through the first hole already made, and denominated 0 upon the hoop; then, bringing the gaged edges of the steel plates to be even with the upper or working side of the hoop, you pinch them together in the vise, and drill and broach the hole through the steel plates, which will make the hole, number 60, upon the hoop. This done, you put the pin through the left-hand hole of the chaps, and the hole marked 0 upon the hoop-plate first made, and proceed to drill with the chaps to 59 holes inclusive, which will fill up the whole space from 0 to the 60th division before obtained.

You now again have recourse to the straps, and placing them one on each side the hoop-plate, you put the pin through the 120th hole of the straps, and through the hole marked 0 upon the hoop-plate; and regulating the steel plates to the hoop-plate as before, you drill and form a hole with the steel plates, which will correspond with the 120th hole upon the hoop-plate; and afterwards filling up the 59 holes wanting, by means of the chaps, you then have all completed to the 120th division, which is one-twelfth of the whole circle.

You then proceed, in like manner, with another set of 120 holes; that is, placing the 60th hole of the straps to the 120th hole of the hoop-plate, and from it producing the 180th hole; you, in like manner as before, fill up this 60 with the chaps; and afterwards placing the 120th hole on the straps in the 120th hole on the hoop-plate, you will obtain the 240th hole; so that filling up this last set of 60 divisions, you have obtained 241 holes, including 240 spaces or divisions of the hoop; and repeating this process ten times more, you will, in like manner, obtain 1441 holes, comprehending 1440 spaces*. And this process being carried on in temperate weather, the manner of working produces twelve similar operations, wherein the materials and tools concerned will not only be subject to very little change of temperature, but that change, whatever it is, will be nearly similar in each set of 120 holes: we may therefore infer, that the greatest inequality, or indeed any that can be sensible, must be at every 60 divisions, that is, between the 59th and 60th, and between the 119th and 120th, both which will be equally repeated 12 times, in the whole length which is to compose the *circumference of a Circle*, and which will thus be checked thereby 12 times in the circumference, and 12 times more at the intermediate distances; that is, with 12 master checks, and 12 subordinate ones, in the whole round.

It is proper here to observe, that in M. ROEMER's method even sixty divisions could scarcely be trusted in an affair of great accuracy, on account of the objections already made, arising from the points having such slight hold in the surface of the brass; but here the parts are held so exceedingly firm, and the

* It will be proper, for reasons hereafter to be mentioned, to continue the divisions to 20 holes more, making in the whole 1461 holes.

operation carried on with so much power, that any small inequality in the hardness of the brass, or irregularity of surface, cannot be supposed to affect the place of the center of the hole; nor will any small inequality that may be suspected from the wear of the steel holes sensibly affect the *center* of the hole, to which every thing is ultimately referred.

Method of joining the Hoop.

A more happy thought than that of HINDLEY's, for joining the two ends of the hoop, could scarcely have been wished for, in regard to preserving the same equality of the space between the holes contiguous to the joint, as in the other parts: for though, geometrically speaking, the two *saddle* plates, in which the little cylindrical bolts are fixed, for bringing the terminating holes of the hoop plate to their due distance, being one applied within the hoop, and the other without, will belong to circles of different *radii*; yet this difference being exceedingly small in such thin metal, and so great a radius, and one being as much too big for the hoop as the other is too little, when the bolts are put in, and the hoop in that part set nearly to a circle by a mould; the mean between them assumed by the hoop, from the elastic compressibility of the materials, will be the truth.

It must, however, be remarked, that in the use of the straps, the joining of the hoop should not be made at any part betwixt an 119th and an 120th division, as some inequality must be supposed there, unless the saddle plates were adapted thereto. The method the most easily practised, will be to continue the division upon the hoop, about twenty more than the completion of the number intended to form the circle, and to cut off all the overplus ones at the beginning.

The

The saddle plates I would recommend to contain ten holes each ; so that if the divisions are carried on to twenty more than what will be contained in the circle, there will be a piece containing twenty to cut off ; and this again being cut in the middle will afford ten holes to make each saddle plate ; so that there will be a place for a bolt on each side the joint, and then putting a bolt through every other hole, there will be three bolts at an end.

The pieces destined for the saddle plates, thus obtained, being broader than can be admitted when put to this use, I would advise to divide the breadth of the plate into three equal parts ; and with a cutting hook (which perhaps will be attended with the least violence in the separation) to separate the two outside pieces from the middle piece : by this means the two saddle plates (though double) will occupy one third only of the breadth of the hoop in the middle ; and two of the pieces cut off being applied, one on each side of the saddle plate on the outside, will answer in like manner for the *rivet* plates.

The last operation to compleat the joining of the hoop is the putting on the rivet plates : to compleat this, I would advise a piece of brass, of three or four inches in length, to be filed so as to answer to the inside of the hoop, when reduced to a true circular form ; and being three-eighths, or one-half an inch in thickness, to file the opposite side somewhat nearly concentric thereto ; apply the middle of its convex arch to the inside of the hoop at the joint, and then bringing on the middle of one of the rivet plates to the joint of the hoop, confine the three together by a couple of narrow-chapped hand vices, leaving a space between them capable of receiving a couple of pins as rivets on each side the joint ; the holes for the rivets are then to be drilled through all, and a little smoothed with a broach at their
their

their entry, into which smooth taper pins are to be driven ; not with violence, but moderately, that no sensible stretching of the solid parts may take place thereby ; then cutting off and smoothing the heads, shift the vices so as to receive another couple of holes, and a third couple in the same end of the hoop ; and proceed progressively in the same manner, from the middle to the other end of the rivet plate ; then gently separate the internal brass mould with a thin knife, or such like instrument ; and cutting off, and very lightly rivetting the inner ends, proceed to fix the other rivet plate, in the same manner, on the other side : by this means the hoop will be firmly joined in the very position given it by the saddle plates and mould. These plates may then be removed, the inside of the hoop cleared and smoothed, if necessary ; and the outside will have the middle part clear where the divisions lie, and that without sensible loss or gain in the juncture.

Method of transferring the Divisions of the Hoop to a dividing Plate.

The hoop being thus refitted for the chock, that should be turned down to leave a shoulder on one side, that the hoop, now reduced to an equal breadth, may be forced against it ; and the divisions, being equally distant from one of its edges, will be all found in a circle, as if turned upon it. It should be very carefully fitted to the chock, that it may go on with a sufficient degree of tightness, and without the necessity of much forcing ; and it will be no inconvenience now, if it goes on upon a very slight degree of taper of the chock, as the internal spring of the materials will easily accommodate it to this shape without any injury to its general truth : a slight degree of a groove should
be.

be turned in the place where the divisions will come, that any conical pin, that is to serve as an index, let drop into the divisions or holes, may not, by reaching through this thin plate, abut upon the wood, rather than upon the sides of the holes : and thus this hoop is made into a wheel of 1440 equal divisions, moveable round upon its own axis, whereon it was formed.

Against the time that this is compleated, there must be prepared a flat circular plate or wheel of brass, the rim of which should be of about $3\frac{1}{2}$ inches breadth, and about two-tenths of an inch in thickness when finished, to make a dividing plate ; the external diameter of this is to be such, that when laid flat upon the surface of the mahogany platform, its extreme edge will exceed the diameter of the hoop by about half an inch all round. There must also be prepared brass arms (suppose eight in number) of an equal substance with the outer rim, and all connected with a circular plate in the middle ; and, the whole of this work being framed beforehand, is to be let on flat upon the mahogany platform ; whose face is supposed to be turned truly flat, and sufficiently affixed with screws : in this situation, the outward edge is to be turned, and the outward face of the rim turned flat. The *center plate*, which may be about twelve inches diameter, is also to be turned as flat as possible, and a center hole, of about half an inch diameter, to be very carefully turned therein.

A piece of clean, straight-grained, well seasoned mahogany, of about two feet long, three inches thick, and five or six inches broad, is then to be well affixed to some part of the general frame of the lathe, which must now have its position altered, so that the platform will become horizontal ; and therefore the frame should be

be originally made with this view *. The piece of mahogany is to be affixed so that one of its larger faces shall be in a parallel plane to the face of the platform, and so low as to clear the under side of the platform in its rotation; and so far distant from the center, that an index may be fixed upon this upper face of the piece of wood, so as conveniently to drop into the holes of the hoop; while the common *cutter frame* of a clock-maker's engine shall be firmly attached upon the same face of the wood, and so fixed as to cut the edge of the dividing plate into teeth, answerable to the several divisions of the hoop. The teeth need only to be cut with a common cutter, making a parallel notch: and here it will be proper to observe, that not only both the index and cutter are to be founded on the same piece or base of wood; but that the nearer they are together, the more free they will be from the effects of all variations of expansions by variations of temperature †.

The equalising the Teeth of the dividing Plate by grinding.

The object of transferring the divisions of the hoop to the teeth of the dividing plate, is still farther to equalise the teeth by grinding; especially those that, falling within the compass of

* After changing the position of the lathe, the collar of its mandrel should be removed, and the neck made to move within three planes, so as to preserve an exact center, in the manner of an *equal altitude* instrument.

† It is proper to observe, that as it may be impracticable to get the rim of the dividing plate cast of the proper size, in one entire piece, it will be very practicable, if cast of a less size (suppose half), but of a sufficient thickness, to roll it down; and by having the outward edge originally thicker than the inner, in the proportion of the *radii*, it may be so managed by the rollers as to be of an equal thickness when brought to its proper size. But the arms and center plate should be of the same metal, rolled in the same degree.

each set of 120 divisions, may be supposed, if any, to be mended thereby; but as it may be incommodious to construct a curved screw, of such a length and size, in HINDLEY's method, as would be sufficient for the purpose, I would propose to use two screws of brass, cut from a cylinder in the way set forth by Mr. RAMSDEN, each of which, with a very little grinding upon this large circumference, would lay hold of ten or twelve teeth together. I would place the two screws, that is, their middles, to be ninety divisions asunder; of consequence, when one of the screws is between the 59th and the 60th, or between the 119th and 120th division of each set, the other will be in the middle of the space divided by the chaps only*.

The threads of these screws I would advise to be cut a little taper, so that as they grind in, they may fill the notches of the teeth; which also, by this means, will acquire a little tapering towards their extremities; and by cutting the notches parallel, as I have mentioned, the true ground part will always be certain of being at the extremity.

When the screws have been used in grinding till they are found to have the effect of a perfectly equal and easy rotation all round, and all the teeth reduced to a sensible taper, and regular bearing, I would then totally remove the screws from the square block of wood, upon whose upper face I suppose them to have been mounted; in like manner as I suppose the index and cutting frame to have been removed, to make room for the mounting of

* The best way of giving an equal motion to those two screws, seems to be by a detached axis, carrying two common flat wheels; one acting upon a like flat wheel, upon the axis of one screw, and the other, in the same manner, upon the other; and applying the pulley for communicating the power to the middle of the detached axis between the two wheels, the spring or twist will be equal both ways; so that in turning the contrary way round, they will still be in equal advance.

the screws. I now consider the teeth of the dividing plate, so formed, as having all the equality that the present known state of human art has pointed out; and the whole convertible upon the axis or mandrel upon which it has been originally formed, and the central hole of the plate concentric therewith: I therefore consider the ground faces of the teeth of the plate as the actual divisions. It now remains to shew how they are to be transferred, to form the divisions of an instrument.

Preparation of the dividing Plate for graduating Instruments.

If a small cylinder of hard steel is duly polished, and made of a size so as just to chock in betwixt the extremities of the teeth, then the center of that cylinder will be a fixed point, in respect to the circumference of the wheel: if another cylinder is applied in like manner, at the distance of a number of divisions (suppose it a prime number, so as to cross all former divisions, viz. 17 or 19), then the middle of the line joining the centers of the two cylinders will remain in the direction of the *same radius*, though one of them should force in a minute quantity further than the other; and if a point is assumed in the direction of a tangent to a circle at this middle-point, then though both the cylinders should drop in a minute quantity further at one time than another, yet the middle-point would remain at the same distance from the point in the tangent; provided that point was removed to a competent distance, that is, to five or six inches. On this principle I would construct an index, the two cylinders being fixed in a frame, convertible about the middle-point, and to be centered in the end of the lever, representing the tangent; then this lever being again convertible about the point in the tangent line, the middle-point would always have a fixed distance

from the point in the tangent, and there hold it steadily fast; the tangent point being placed upon the fixed block before-mentioned.

Use of the dividing Plate in the Graduation of Instruments.

Our dividing plate is now ready for the reception of an instrument; suppose it a quadrant, whose radius, however, must not exceed the radius of the dividing plate: It is to be laid upon the face of the dividing plate, and a weight, or weights, equivalent to that of the quadrant, is placed on the opposite side, to balance it. It must also be supposed, that the quadrant is made with a view to be divided by this engine; and consequently, that the central cylinder is so well adapted, and nicely fitted to the center hole of the quadrant, that the center cylinder can be removed, in order for the limb to be divided, and again replaced, without sensibly altering its center. This being the case, let a piece of metal be turned, to apply to the quadrant, perfectly like its center cylinder at the upper end, and turned nicely to fit the central hole in the dividing plate, at the lower end; then, the quadrant being fixed with proper fastening screws, I would cut the divisions with a beam compass; and, if a fixed point is assumed, *viz.* the center of the tangent point for the index; then the beam compass being always opened to the computed length of the tangent of the circle of divisions, it will be sufficiently near for cutting the divisions, square to the circular arches between which they are placed.

It will also be proper (to prevent unequal expansions) that the beam of the compass should be formed of a piece of clean-grained *white fir*; and that the length between the points be inclosed in a tube of tin or brass; without touching the beam, except

except at the terminations, which will in a great measure protect it from both alteration of moisture, and of heat from the body of the artist, during the operation.

It will be likewise proper to have a lever, or some equivalent contrivance, to bring the dividing plate forward; that after lifting the little cylinders out of the divisions, and resting them upon the tops of the teeth, they may be brought gently forward with an equal drag, and ultimately snap in between the teeth, by the strength of the spring commanding the index; by this means the drag of the friction of the whole will be constantly the same way.

Conclusion.

Now, if, as it has been shewn, a quadrant of any radius may be read off to the 40000th part of an inch, then this quantity upon a radius of three feet will not be so much as $1\frac{1}{2}$ second; and as the whole of the process is carried on by contact, in which a greater error than that of a 60,000th part of an inch cannot be admitted in any single operation, I should assuredly expect a three-feet quadrant, so divided, to be true in its divisions, and read off to at most two seconds.

But, after all, in an instrument like this, I should expect the greatest source of error to be in the want of perfect coincidence of the center of the divisions with the actual center upon which the index revolves; and therefore, that if, instead of a quadrant of three-feet radius, a complete circle of five feet diameter was divided, and its divisions read off from the two opposite points (taking the mean), then the errors of the center will be wholly avoided. For this reason, I am very clearly of opinion, that the sagacious proposition of Mr. RAMSDEN,

DEN, to use circles instead of quadrants, or other portions of circles, will bid much the fairest for perfection in actual practice; and that his ingenious method of making them both stiff and light, by the use of hollow conical tubes by way of spokes, in the manner of a common wheel, will enable him to mount them of five feet diameter, upon hollow axes, in the nature of a *transit*. By this means we shall have all the good properties of both the quadrant and transit united in one instrument; and observations both of right ascension and declination, through the very same telescope, as long since attempted by M. ROEMER; and to a degree of perfection and certainty, in point of declination, hitherto unattainable by the largest instruments that have yet been made.

N. B. In matters of very nice determination, small circumstances often come to be of consequence; and it is in this view that I mention what follows. It was a practice of HINDLEY's of many years standing, and since followed by myself and others, wherever he made any use of the *vernier*, to lay the vernier plate in the same plane, or cylindrical surface continued, whereon the principal divisions are cut. It is of equal utility, though the vernier be rejected, to lay the index stroke in the plane of the divisions. In this way the divisions being by convenience upon the external border of the limb*, ~~some~~ sets of divisions are thereby rendered inconvenient; but those

* It has been objected, that laying the divisions upon the extreme edge of the limb of the instrument subjects it to injury: but, to obviate this, in an HADLEY's quadrant made for me, by my direction, by the late Mr. MORGAN, in the year 1756, wherein the vernier is laid even with the divisions, those are protected by a projection of the solid part of the limb, beyond the divisions; a *Roller* being sunk in the edge of the limb, to clear the vernier.

that

that with two sets, as a check, will in a great measure aid themselves, by reading from two different parts of the same set of divisions; which is very easily provided for, by putting an additional stroke upon the index plate, at the distance of 9, 11, or any prime number of divisions to 19, 23, or more; and reading off from that stroke also; as before recommended for great quadrants, where the vernier is proposed to be rejected *: so that they will thereby be mutually checked by divisions that had no correspondence in their original formation.

* I would not have it thought, from my proposal of rejecting the vernier, that I have any quarrel with it; I think it a very simple and ingenious contrivance, where it is properly applicable; that is, where the strokes of the vernier, or their estimated halves, are sufficient for all the precision required or expected from the instrument, as in HADLEY's quadrants, theodolites, &c.: but where still more minute divisions are required than can easily be had by estimation from the vernier; to do this by a screw, as a *supplement* to the vernier, appears to me in the light of bringing a more accurate tool to supply the deficiencies of one less accurate; when the former might, with more propriety, supply the place of the latter altogether.

11. *A Series of Observations on, and a Discovery of, the Period of the Variation of the Light of the Star marked δ by Bayer, near the Head of Cepheus. In a Letter from John Goodricke, Esq. to Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read November 24, 1785.

S I R,

York, June 28, 1785.

THE improvements which of late years have been introduced into astronomy, should be attributed not only to the diligence and accuracy wherewith astronomers prosecute their observations and discoveries, but in part also to your exertions, and especially to that kind encouragement which you have, on many occasions, afforded those who make this science their chief study; and I am happy to have this opportunity of acknowledging myself one of those who are much indebted to you in this respect. Under these impressions I thought I could not do better than to address to you the following account of a periodical variation in the star δ Cephei, which I lately discovered. This account will, I presume, be a considerable addition to the few discoveries that have but very lately been made respecting the same subject. They may probably lead to some better knowledge of the fixed stars, especially of their constitution and the cause of their remarkable changes.

My

My first observation was Oct. 19, 1784; and as I wished to establish the several points of the variation with as great accuracy as the nature of the subject will admit of, I have delayed sending this account till now; but as observations made through so long an interval of time must be very numerous, and would only swell this paper to an unnecessary length, I have in the following series formed a selection, chiefly of those that were made under the most favourable circumstances; and I must add, that none of those that are omitted contradict the results. From this series I have settled, that the star has a periodical variation of 5 d. 8 h. 37 $\frac{1}{2}$, during which time it undergoes the following changes:

1. It is at its greatest brightness about one day and thirteen hours.
2. Its diminution is performed in about one day and eighteen hours.
3. It is at its greatest obscuration about one day and twelve hours.
4. It increases in about thirteen hours.

When it is in the first point it appears as a star of between the fourth and third magnitude; but its relative brightness does not seem always to be quite the same, being sometimes between ζ and ι Cephei, and sometimes only equal to, or something less than, ι Cephei, or between ζ Cephei and γ Lacertæ. In the third point it appears as a star of between the fourth and fifth magnitude, if not nearer the fifth; and its relative brightness is as follows: nearly equal to ϵ and ξ Cephei, and considerably less than γ Lacertæ.

The relative brightness and magnitude of those stars to which the variable one was compared, is as follows: ζ Cephei, the brightest, is between the third and fourth magnitude; ι Cephei,

the next brightest, is between the fourth and third; γ Lacertæ is less than ι Cephei, and of about the fourth magnitude; ϵ Cephei is between the fourth and fifth magnitude; and ξ Cephei, which is a little less than ϵ , is between the fifth and fourth.

A Series of Observations on the Variation of the Light of the Star δ Cephei.

1784, Oct. 19, at $8\frac{1}{2}$ h. I thought it was rather less than ζ Cephei.

Oct. 20, at $8\frac{1}{2}$ h. it was rather less than ζ , and about equal to ι Cephei.

Oct. 22, at $9\frac{1}{2}$ h. less than ι , and larger than ϵ Cephei; but the air was not very favourable.

Oct. 23, at $6\frac{1}{2}$ h. and 11 h. less than γ Lacertæ, and a little brighter than ϵ Cephei.

Oct. 24, at $6\frac{1}{2}$ h. less than ζ Cephei, somewhat less than ι Cephei, and something brighter than γ Lacertæ; strong moon-light, and air rather hazy.

At $8\frac{1}{2}$ h. to 11 h. a little less than ζ and ι Cephei, and brighter than γ Lacertæ; air clear and frosty; the moon was very low at 11 h.

Oct. 25, at 6 h. 8 h. and 11 h. nearly the same; air pretty clear, and moon bright.

Oct. 26, at $9\frac{1}{2}$ h. and 11 h. rather less than γ Lacertæ; strong moon-light, but air very clear.

Oct. 27, $6\frac{1}{2}$ h. and $10\frac{1}{2}$ h. less than γ Lacertæ, and brighter than ϵ Cephei; ditto.

Oct. 28, at 9½ h. and 12 h. just the same, if not less; moon-light, but the air was remarkably clear.

Oct. 31, at 8 h. nearly equal to, if not less than, 7 Lacertæ.

Nov. 1, at 11½ h. somewhat less than 7 Lacertæ; air clear.

Nov. 3, at 12¼ h. equal to, if not a little less than, 7 Lacertæ; but the weather was not very favourable: it seemed to have increased since my first observation, which was at 5½ h.

Nov. 5, at 13 h. brighter than 7 Lacertæ, and less than ζ Cephei; flying clouds, but air pretty clear.

Nov. 6, at 9 h. and 12¼ h. rather less than 7 Lacertæ.

Nov. 7, at 7½ h. I thought it still rather less than 7 Lacertæ, but at 10½ h. and 11 h. it was evidently less than it; air clear.

Nov. 10, at 11 h. and 12¼ h. something less than ζ Cephei, and brighter than 7 Lacertæ; clear sky.

Nov. 11, at 7 h. to 12 h. a little brighter than 7 Lacertæ.

Nov. 12, at 7 h. and 8½ h. about equal to 7 Lacertæ. From 9½ h. to 12¼ h. it was something less than 7 Lacertæ, and brighter than ε Cephei.

Nov. 13, at 6½ h. to 11 h. only a little brighter than ε Cephei, though sometimes it appeared equal to it.

Nov. 14, at 7½ h. brighter than ε Cephei, and, I believe, equal to 7 Lacertæ. There was a haziness about 7 Lacertæ.

Nov. 15, at 12 h. less than ζ Cephei, and brighter than 7 Lacertæ; fine *aurora borealis*, but the air was very clear.

At 18½ h. ditto; but the air was not very clear.

Nov. 16, at 6½ h. and 10 h. just the same, if not decreased at 10 h.

Nov. 17, at 6½ h. to 10½ h. a little less than 7 Lacertæ, and brighter than ε Cephei; air clear.

Nov. 18, at 9 h. to 12 h. and 19 h. little brighter than ε and ξ Cephei.

Nov. 19, at 6 h. to 10 h. just the same, being but a very little brighter than ϵ and ζ Cephei; air clear.

At 18 $\frac{1}{2}$ h. it was increased, being now brighter than ϵ and ζ Cephei.

Nov. 20, 7 h. to 11 h. considerably brighter than γ Lacertæ, something less than ζ Cephei; air extremely clear at 11 h.

Nov. 21, at 6 h. exactly the same.

Nov. 22, at 9 $\frac{1}{4}$ h. about equal to γ Lacertæ; moon-light.

Nov. 25, at 7 h. and 8 h. less than γ Lacertæ, and brighter than ϵ and ζ Cephei; air clear.

At 9 $\frac{1}{2}$ h. and 9 $\frac{3}{4}$ h. a little brighter than γ Lacertæ.

At 10 $\frac{1}{2}$ h. and 12 h. brighter than γ Lacertæ, and about between ζ Cephei and γ Lacertæ, but rather nearer γ Lacertæ; air clear and moon-light.

Nov. 26, at 9 h. exactly as last night.

Nov. 29, at 7 $\frac{1}{2}$ and 8 h. less than γ Lacertæ, and something brighter than ϵ and ζ Cephei.

Nov. 30, at 8 $\frac{1}{4}$ h. as last night; air clear.

At 10 $\frac{1}{4}$ h. between γ Lacertæ and ϵ Cephei, but nearer ϵ .

At 10 $\frac{1}{2}$ h. 11 h. and 12 h. ditto, but nearer γ Lacertæ; air clear. I have no doubt of its increase since 8 $\frac{1}{4}$ h. Mr. E. PIGOTT found it rather less than ζ Cephei at 18 $\frac{1}{2}$ h. *See his Observations.*

Dec. 1, 11 h. something less than ζ Cephei, and brighter than γ Lacertæ.

Dec. 3, 12 $\frac{1}{4}$ h. less than γ Lacertæ, and brighter than ϵ Cephei.

Dec. 4, 5 $\frac{1}{2}$ h. to 12 h. little brighter than ϵ and ζ Cephei.

Dec. 7, 10 h. and 11 h. between ζ Cephei and γ Lacertæ.

Dec. 8, at 10 $\frac{1}{2}$ h. between γ Lacertæ and ϵ Cephei.

Dec.

Dec. 9, $11\frac{1}{2}$ h. ditto, but nearer ϵ Cephei; about equal to ξ Cephei.

Dec. 11, 6 h. something less than γ Lacertæ; brighter than ϵ Cephei.

At $7\frac{1}{2}$ h. something brighter than γ Lacertæ.

At $8\frac{1}{4}$ h. brighter than γ Lacertæ.

At 9 h. and 11 h. between ζ Cephei and γ Lacertæ, but nearer γ Lacertæ.

Dec. 12, at 6 h. something less than ζ Cephei.

Dec. 13, at $9\frac{1}{4}$ h. brighter than γ Lacertæ; considerably less than ζ Cephei.

Dec. 14, at $8\frac{1}{2}$ h. nearly equal to, if not less than, γ Lacertæ.

Dec. 17, at $5\frac{1}{2}$ h. and $7\frac{1}{2}$ h. equal to, if not less than, ϵ Cephei, and between ζ Cephei and γ Lacertæ, but nearer ζ .

Dec. 18, at 9 h. less than ϵ Cephei, and between ζ Cephei and γ Lacertæ, but nearer γ Lacertæ.

Dec. 19, at 19 h. less than γ Lacertæ; considerably brighter than ϵ and ξ Cephei.

Dec. 20, at 6 h. and 7 h. about equal to ξ Cephei, and a little brighter than ϵ Cephei.

Dec. 21, at 8 h. and 18 h. nearly equal to ϵ Cephei.

Dec. 22, at $8\frac{1}{4}$ h. considerably brighter than γ Lacertæ, less than ζ , and a little less than ϵ Cephei; strong moon-light.

Dec. 25, at $5\frac{1}{2}$ h. between γ Lacertæ and ϵ Cephei.

Dec. 28, at 8 h. &c. between ζ Cephei and γ Lacertæ, and equal to, if not less than, ϵ Cephei.

Having, in the beginning of this paper, mentioned my intention of omitting several observations, in order to be as short as possible, I have thought it best, with the exception of one only, to leave out all that were made in January, February, and

and March, because they were much interrupted by the then unfavourable state of the weather.

1785, Feb. 8, at 9 h. equal to 7 Lacertæ; considerably less than ι Cephei.

At 10 h. rather brighter than 7 Lacertæ.

At 11 h. brighter than 7 Lacertæ; a little less than ι Cephei.

April 1, at 11 h. about equal to ϵ and ξ Cephei; weather not favourable.

April 2, at 12½ h. ditto.

April 3, at 8 h. a little less than ι Cephei, less than ζ Cephei, and brighter than 7 Lacertæ.

April 4, at 12 h. ditto; if any thing, it is less than it was last night.

April 7, at 10 h. about equal to ϵ and ξ Cephei; but the weather was not very favourable.

April 8, at 7½ h. considerably less than ι Cephei, brighter than ϵ and ξ Cephei; but the air was not very clear.

At 10 h. it was increased.

At 11 h. only a little less than ι Cephei.

At 12 h. equal to, if not a little brighter than ϵ , and less than ζ Cephei; considerably brighter than 7 Lacertæ.

April 12, at 12 h. a little less than ϵ , and nearly equal to ξ Cephei.

April 13, at 9 h. just the same.

At 11 h. seemed rather increased, being equal to ϵ , and a little brighter than ξ Cephei.

April 16, at 11½ h. nearly equal to ϵ and ξ Cephei.

April 17, at 9 h. and 11 h. rather a little less than ϵ , and a little brighter than ξ Cephei.

April

April 19, at 11½ h. about equal to ι Cephei, if not a little brighter than it; less than ζ Cephei, and considerably brighter than γ Lacertæ.

April 24, at 10 h. a little brighter than γ Lacertæ; considerably less than ι Cephei.

At 12 h. scarce at all altered, but if any thing it is a little increased; air very clear, and observation good.

April 25, at 10 h. and 11½ h. little less than ι Cephei, and considerably brighter than γ Lacertæ.

April 26, at 10 h. and 11 h. less than γ Lacertæ, something brighter than ϵ , and brighter than ξ Cephei.

May 4, at 9½ h. and 12 h. a little less than ϵ and ξ Cephei.

May 7, at 12 h. less than ι Cephei, and a little brighter than γ Lacertæ.

May 9, at 11 h. a little less than ϵ Cephei.

May 10, at 12 h. between γ Lacertæ and ϵ Cephei, but something nearer ϵ .

May 11, at 10 h. and 12 h. brighter than ι Cephei, less than ζ Cephei, and much brighter than γ Lacertæ.

May 14, at 11½ h. much less than γ Lacertæ, equal to, if not a little brighter than, ϵ Cephei, and brighter than ξ Cephei.

May 15, at 9½ h. less than ϵ , and about equal to ξ Cephei.

May 19, at 9½ h. and 11 h. equal to, if not a little brighter than, ϵ Cephei, and brighter than ξ Cephei.

May 20, at 9½ h. 11 h. and 12 h. a little less than ϵ , and nearly equal to ξ Cephei.

May 21, at 12 h. equal to, if not a little less than, ι Cephei; less than ζ Cephei, and considerably brighter than γ Lacertæ.

May 22, at 10 h. and 11½ h. a little brighter than ι Cephei, the rest as last night.

May 23, at 11 h. and 11½ h. nearly equal to γ Lacertæ, and less than ι Cephei.

May 25, at 10 h. and 12 h. a little less than ϵ , and about equal to ξ Cephei.

May 27, at 10 h. between ζ and ι Cephei, and considerably brighter than γ Lacertæ.

May 28, at 12 h. between ι Cephei and γ Lacertæ.

June 1, at 9½ h. I thought it less than ι Cephei; air not clear, and twilight pretty strong.

At 10½ h. and 12 h. between ζ and ι Cephei, but rather nearer ι .

June 2, at 12 h. exactly the same.

June 6, at 12 h. ditto; the weather was not very favourable, but the observation seemed good.

June 10, at 11½ h. a little less than ϵ Cephei.

June 12, at 11 h. between ζ and ι Cephei.

June 21, at 10 h. nearly equal to, if not a very little brighter than, ϵ Cephei; twilight.

At 11½ h. a little less than ϵ , and about equal to ξ Cephei.

June 23, at 11½ h. between ζ and ι Cephei, and brighter than γ Lacertæ.

June 24, at 11½ h. ditto; only a short view.

June 25, at 11½ h. a little, but certainly, brighter than ϵ Cephei, brighter than ξ Cephei, and considerably less than γ Lacertæ.

June 26, at 11½ h. a little less than ϵ Cephei, and equal to, if not a little brighter than, ξ Cephei.

In the above collection I find only two or three mistakes of any consequence, *viz.* the dates of the observations of April 7, and 8, are marked in my journal for April 8, and 9; but I have corrected them, being convinced they are erroneous: and the observation of May 10, I think, disagrees rather too much from what it ought to be by computation.

The

The following observations were made by my friend Mr. E. FIGOTT; who, at my request, was so kind as to observe the star as often as possible, though then in an ill state of health. They are, I presume, sufficient to corroborate the variation of the star as above stated, although in one or two places there may be found some little differences between our observations.

MR. FIGOTT'S OBSERVATIONS.

1784, Oct. 25, at 12 h. rather brighter than γ Lacertæ; much brighter than ϵ Cephei, and much less than ζ Cephei; nearly between ζ Cephei and γ Lacertæ.

Oct. 26, at 12 h. seemed the same as yesterday.

Nov. 1, at 12 h. brighter than ϵ Cephei; seemed rather less than γ Lacertæ.

Nov. 13, at 8½ h. rather, but very little, brighter than ϵ Cephei; less than γ Lacertæ.

Nov. 15, at 12 h. seemed rather brighter than γ Lacertæ, and less than ζ Cephei.

Nov. 17, at 8 h. less than γ Lacertæ; rather brighter than ϵ Cephei.

Nov. 18, at 12 h. equal to ϵ Cephei, though sometimes it seemed less; less than γ Lacertæ.

Nov. 19, at 12 h. seemed equal to ϵ Cephei.

Nov. 20, at 11 h. rather less than ζ Cephei; brighter than γ Lacertæ.

Nov. 25, at 11½ h. if not equal rather brighter than γ Lacertæ; much brighter than ϵ Cephei.

Nov. 29, at 8 h. equal to ϵ Cephei.

Nov. 30, at 11½ h. brighter than ϵ Cephei; less than γ Lacertæ.

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At

At $18\frac{1}{2}$ h. much increased; rather less than ζ Cephei.

Dec. 4, at $6\frac{1}{2}$ h. sometimes thought it less, and at other times brighter, than ϵ Cephei.

Dec. 11, at $5\frac{1}{2}$ h. less than γ Lacertæ; rather brighter than ϵ Cephei.

At $11\frac{1}{2}$ h. rather brighter than γ Lacertæ; not at its full brightness.

Dec. 21, at 7 h. if any difference less than ϵ Cephei.

At $18\frac{1}{2}$ h. a little brighter than ϵ Cephei.

Dec. 22, at 8 h. less than ζ Cephei; a little brighter than γ Lacertæ.

Dec. 28, at $5\frac{1}{2}$ h. nearly equal to ζ Cephei; had only a short view of them.

1785, April 26, at $11\frac{1}{2}$ h. less than ζ , rather less than ϵ Cephei, brighter than ϵ Cephei, and if any difference rather brighter than γ Lacertæ.

May 4, at $9\frac{1}{2}$ h. much less than ζ Cephei, less than ϵ Cephei, and than γ Lacertæ, and rather brighter than ϵ Cephei.

May 7, at 11 h. rather less than ϵ Cephei, and brighter than ϵ Cephei.

May 9, at $11\frac{1}{2}$ h. rather brighter than ϵ Cephei, and much less than ϵ Cephei, and γ Lacertæ.

May 11, at $10\frac{1}{2}$ h. rather less than ζ , and rather brighter than ϵ Cephei; much brighter than γ Lacertæ.

May 19, at 10 h. equal to ϵ Cephei, but if any difference rather brighter; little hazy and moon-light. The same at 12 h. but the weather was not hazy then.

May 20, at $11\frac{1}{2}$ h. and $12\frac{1}{2}$ h. rather brighter than ϵ Cephei, and much less than γ Lacertæ; moon-light strong at $11\frac{1}{2}$ h.

May 21, at $12\frac{1}{2}$ h. equal to γ Lacertæ; less than ϵ Cephei.

May

May 22, at 12½ h. equal to, if not brighter than, γ Lacertæ; think it brighter than ι Cephei.

May 23, at 11½ h. seemed sometimes equal to, though generally less than, ι Cephei and γ Lacertæ.

Having now delivered the observations, from whence I have deduced the preceding conclusions, nothing more relative to this subject remains to be mentioned, except the determination of the period; in the doing of which I must follow nearly the same methods as have been used in some preceding papers. It is very evident, from a rough calculation, where only single periods or very short intervals are used, that it is about five days and eight hours. In order to determine this period with greater exactness, I have, in the following table, collated some of the most precise phases. The first five are times when δ Cephei was observed to be equal to γ Lacertæ during the course of its increase of brightness, which proceeds rapidly. The five next are similar times, with this only difference, that as it was not then actually observed to be equal to γ Lacertæ, a proper allowance from the nearest observations was made on supposition that the changes are similar in every period. The ten last are assumed times between its least and greatest brightness, which determinations can hardly err more than a few hours, as the whole increase is completed in thirteen hours; but even were it so, the periods deduced from them would still be exact, because the intervals are very long.

1784 and 1785.

D.	H.				D.H.M.
Nov. 25,	8 $\frac{1}{2}$	} an interval of 14 periods each of			5 8 34+
Feb. 8,	9				
Dec. 11,	6 $\frac{1}{2}$	} ditto	11	ditto	5 8 54 $\frac{1}{2}$
Feb. 8,	9				
Nov. 25,	8 $\frac{1}{2}$	} ditto	25	ditto	5 8 36
April 8,	8				
Nov. 30,	16	} ditto	24	ditto	5 8 40
April 8,	8				
Nov. 30,	16	} ditto	27	ditto	5 8 35 $\frac{1}{2}$ -
April 24,	8				
Oct. 23,	21	} ditto	39	ditto	5 8 41 $\frac{1}{2}$ +
May 21,	0				
Nov. 19,	22	} ditto	34	ditto	5 8 31 $\frac{1}{2}$
May 21,	0				
Dec. 22,	1	} ditto	28	ditto	5 8 32 +
May 21,	0				
Nov. 19,	22	} ditto	25	ditto	5 8 40 $\frac{1}{2}$
April 2,	23				
Nov. 19,	22	} ditto	6	ditto	5 8 50
Dec. 22,	1				

Hence the period is, on a mean,

5 8 37 $\frac{1}{2}$ +

A few cursory remarks shall conclude this Paper. What I have before mentioned, that the greatest brightness of δ Cephei does not seem to be always quite the same, is not peculiar to this star, but is also to be observed in the other variable ones. I have remarked in a late Paper, that the greatest brightness of β Lyræ is subject to considerable alterations, and thought then that it might be owing to some fallacy of observation; but now

I have reason to alter, in some measure, my opinion on this head. Even Algol does not seem to be always obscured in the same degree, being perceived to be sometimes a little brighter than ϵ Persei, and sometimes less than it *. These seeming irregularities, however, do not appear to affect the period; for if we compare the same precise phases together, it will be found still regular. This may, I suppose, be accounted for, by a rotation of the star on its axis, having fixed spots that vary only in their size.

I need not say, that the situation of δ Cephei, on account of its great northern declination is such, that its changes may be observed with great advantage in these latitudes, it being always sufficiently elevated above the horizon. To this circumstance are also owing its various changes of position, which, I find, affect the comparative brightness of the stars a little; but, as these differences are very trifling, I shall take no further notice of them.

If you think this account worthy of notice, I beg you will be so kind as to communicate it to the Royal Society.

I remain, with great regard, &c.

JOHN GOODRICKE.

* This will appear from an attentive examination of the observations of that star's diminution in my two late Papers, which were printed in the LXXIII^d and LXXIVth volumes of the Philosophical Transactions. I did not take much notice of it then, because I thought the difference was too small to be relied on; but the observations I have made since seem to confirm that it does *really* diminish a little unequally. M. MECHAIN, in a letter to Mr. E. PIGOTT, mentions the same fact.



III. *Magnetical Experiments and Observations.*

By Mr. Tiberius Cavallo, F. R. S.

(The Lecture founded by the late HENRY BAKER, Esq. F.R.S.)

Read November 24, 1785.

THE object of this lecture is to shew the properties of some metallic substances with respect to magnetism; and the experiments herein related seem to ascertain some new and remarkable facts.

The magnetic properties have been generally thought to belong only to iron, or to those substances which contained that metal; comprehending under the general name of iron not only the metal commonly so called, but likewise its more perfect and more imperfect states, *viz.* steel, iron ores, amongst which is considered the magnet, and the calces of iron, excepting only those which are very much dephlogisticated, for they possess no magnetic property whatsoever. Some other metallic substances, and especially platina, brass, and nickel, on which the magnet has some action, were thought to be magnetic so far as they contained some portion of iron, the presence of which may be manifested by chemical methods in many cases, but not always; because the quantity of iron may be so excessively small in proportion to the weight of the other metal in which it is concealed, as not to be discoverable by chemical analysis, and yet it may be sufficient to affect the magnetic needle.

needle. The following experiment will shew, that an exceedingly small quantity of iron will render a body sensibly magnetic.

Having chosen a piece of Turkey-stone, which weighed about an ounce, I examined it by a very sensible magnetic needle, and found that it had not the least degree of magnetism, the needle not being moved from its usual direction by the vicinity of any part of the surface of the stone; I then weighed a piece of steel with a pair of scales that turned with the twentieth part of a grain, and afterwards drew one end of it over the surface of the stone in various directions. This done, the piece of steel was weighed again, and was found to have lost so small a part of its weight as not to be discernible by that pair of scales; yet the Turkey-stone, which had acquired only that small quantity of steel, affected the magnetic needle very sensibly. Chemistry seems not to afford any means by which so small a quantity of iron may be decisively detected in a body that weighs one ounce. Hence it follows, that though no iron is to be discovered in a body by chemical methods, yet it should not be concluded, that the said body, if it affect the magnetic needle, does not own its magnetism to some small quantity of iron concealed in its substance.

Nickel is a metallic substance which has been suspected to be capable of acquiring some degree of magnetism independent of iron; and this suspicion has been founded upon observing, that nickel retained its magnetism after having been repeatedly purified*. There are, however, persons who have denied the magnetism of purified nickel; and I have seen some pieces of it which did not in the least affect the magnetic needle. It is probable, that those pieces were not pure nickel, and perhaps

* See KRAWAN's Mineralogy, p. 342. and 367.

some

some cobalt was contained in them; but I see no reason why the nickel, when alloyed with a little cobalt, should shew no magnetism, if that property does really belong essentially to it.

The greatest number of my experiments are relative to the properties of brass; and they seem to prove, that this compound metal, which is often magnetic, does not owe its magnetism to iron, but to some particular configuration of its component particles, occasioned by the usual method of hardening it, which is by hammering.

In some specimens of brass, and especially in that which has often passed from the work-shop to the furnace, and from the latter to the former, there are sometimes pieces of iron sensible not only to the magnet, or the chemical analysis, but even to the sight, which render the brass strongly magnetic. But the brass generally used in my experiments was such as, when quite soft, it had no sensible degree of magnetism.

Before we begin with the narration of those experiments, it will be proper to describe the magnetic needle I generally used; which is suspended in a particular manner; and which may be useful to persons who are fond of making magnetic experiments, not only for its sensibility, but likewise for the simplicity of its construction.

Experience having shewn, that large magnetic needles are not proper for experiments wherein a very small degree of magnetism must be ascertained, and the free motion of the usual small needles being proportionally more obstructed by the nature of their suspension, even when furnished with agate caps, I endeavoured to contrive a sort of suspension which might answer the purpose better than the needles suspended in the usual manner; and, after several attempts, at last I constructed a chain
of

of horse-hair, consisting of five or six links, to which the needle was suspended. Each link is about three-quarters of an inch in diameter; and the extremities of each piece of hair, which is formed in a ring, are joined by a knot, and secured by a little sealing-wax. The link on one end of this chain is suspended to a pin in a proper frame, or any support that may be at hand; and to the link of the other extremity which lies lowermost, a piece of fine silver wire is hooked. This wire is about an inch and a half long, and its lower extremity is fastened round a small and cylindrical piece of cork, through which a common sewing needle, made magnetic, is thrust horizontally. Thus the magnetic needle is suspended by a hair-chain, the links of which, on account of the smoothness and lightness of the hair, move very freely in each other, and allow the needle more than a whole revolution round its centre, with so small a degree of friction as may be considered next to nothing. By comparing this needle with others of the best sort in use, I find the former to be much more sensible; for when bodies which have an exceedingly small magnetic power are tried, this needle will be frequently attracted by them when the others are not sensibly affected.

In order to try farther the delicacy of such suspension, I placed a piece of looking-glass under the needle, and nearly horizontal, so that the image of the needle was seen in it. Now, as a fine line had been previously marked on the glass, things were so disposed as that the image of the needle might coincide with the line marked on the glass, the eye being placed in a proper point of view; afterwards, by shaking the needle either very gently or very quickly, I repeatedly endeavoured to place it out of the magnetic meridian; but every

endeavour proved ineffectual, for the needle constantly settled in the same direction, without any sensible variation.

With a needle thus suspended a variation compass might be very easily constructed, and it would perhaps be more accurate than those commonly in use. For this purpose the needle ought to be about three inches long, and the piece of looking-glass ought to be fixed upon the index of an HADLEY's sextant, which must be placed horizontally under the needle, with its edge or fiducial line in the meridian of the place, in order to observe the daily variation of the needle. I have made only a rough model of such a variation compass, and it seemed to answer very well. This construction appears to have the following advantages over the common sort: 1st, the needle being cylindrical, and without a hole in the middle, would be less subject to have more than two poles. 2dly, The needle being slender, its poles would stand more exactly in its axis, which with the common flat needles is seldom the case. 3dly, It will appear, by a little consideration, that in this construction there is no need of the needle's center of motion keeping always in the same invariable point, which renders the construction both very easy and very accurate: and, lastly, as the sextant may be placed at a considerable distance below the needle, and the rest of the frame may be made of any size, there would be no necessity of placing any brass or other metal so near the needle as might affect it in case this metal had any magnetism, which generally happens with brass.

In order to examine the magnetism of divers substances, besides the above described needle, I used to put a small magnetic needle upon water, and then bring the substance to be examined near it, or place the substance itself upon water, sometimes

sometimes resting it upon pieces of cork, and then bring a powerful magnet near it.

Examination of the Magnetical Properties of Brass.

A few years ago, being intent on making some magnetic experiments, in which brass was concerned, I used to examine first whether the pieces of brass had any magnetism or not, and rejected those pieces which had an evident degree of that power. In the course of those experiments I remember to have observed, that those pieces of brass which had been hammered were generally magnetic, and much more so than others; in consequence of which I made no use of hammered brass in those experiments. But lately, having ordered a theodolite at a philosophical instrument shop, I particularly enjoined the workmen to try the brass, both soft and hammered, before they worked it, and to make no use of that which had any magnetism. They found, that hammered brass, even such as before the hammering had no magnetism, could afterwards disturb the magnetic needle very sensibly. These observations induced me to make the following experiments.

E X P E R I M E N T I.

An oblong piece of brass, weighing somewhat less than half an ounce, being examined by presenting every part of its surface to the suspended needle, shewed no sign of magnetism whatever. It was then hammered for about two minutes; the consequence of which was, that it became magnetic so far as to attract either end of the needle from about a quarter of an inch. This same piece of brass being now put into the fire so as

to become red-hot, by which means it was softened, and when cold being presented to the suspended needle, its magnetism was found to be entirely gone. Hammering made it again magnetic. Softening by fire took the magnetism away a second time; and thus the magnetism was repeatedly given it by hammering, and was destroyed by softening; sometimes shewing to have acquired a sensible degree of that power, even after two or three strokes of the hammer.

EXPERIMENT II.

The result of the first experiment would naturally induce one to suspect, that the hammer and anvil might have imparted some small quantity of steel to the brass, which rendered it magnetic; and that this magnetism was destroyed in softening the brass, inasmuch as the fire calcined the small quantity of steel that had adhered to it. In consequence of which consideration, I took other pieces of brass besides that used before, and hammered them between card-paper, changing the pieces of paper as often as was necessary, since they were easily broken by the hammer; but the pieces of brass became constantly magnetic by the hammering, and their magnetism was destroyed by fire.

In this experiment I generally gave to the brass not above thirty strokes with the hammer.

EXPERIMENT III.

Still suspecting that the hammer and the anvil might have imparted some small quantity of iron to the brass, because the pieces of card-paper sometimes were broken by the first or second

second stroke, in which case either the hammer or the anvil touched the brass; I hardened a piece of brass by beating it between two large flints, *viz.* using one for the hammer, and the other for the anvil. The piece of brass became magnetic, though in this case it seemed to have acquired not so much power as when it had been hardened with the hammer; but it must be observed, that the flints being rough and irregular, the piece of brass could not be hardened by them so easily, or so equally, as by the other method.

The flints, being examined both before and after the experiment, were found to have not the least degree of magnetism.

EXPERIMENT IV.

A piece of brass, which by hammering had been rendered so strongly magnetic as to attract either pole of the needle from about a quarter of an inch, was put into a crucible, together with a considerable quantity of charcoal dust, which surrounded it every where. The crucible was covered with clay, and being put into the fire, was kept red-hot for about ten minutes. After cooling, the piece of brass was taken out of the crucible, and being examined, was found to have entirely lost its magnetism. The object of this experiment was to ascertain whether the loss of magnetism, in a piece of brass that was softened, was owing to the calcination of the ferrugineous particles, which, notwithstanding the preceding experiments, might still be suspected to be imparted to it; because in this way of softening the brass, the ferrugineous particles being surrounded with charcoal dust, could not have been calcined; hence the brass ought not to have lost its magnetism, which was not the result of the experiment.

EXPE-

E X P E R I M E N T V.

One of those pieces of brass which had been used for the foregoing experiments, and which had been deprived of magnetism by fire, was hammered between two large and pretty thick pieces of copper, which were not in the least magnetic; and, after a few strokes of the hammer, it became sensibly magnetic.

E X P E R I M E N T VI.

In order to examine the difference of this property in brass of various kinds, I have tried a great many pieces of English as well as foreign brass; some of which was very old, and so fine and uniform, that an eminent watch-maker of my acquaintance used it for the very best sort of watch work. But I find, that they mostly have the property of becoming magnetic by hammering, and of losing that power when softened. There are, however, some pieces which acquire no magnetism by the hammering, though they are rendered equally hard by it as those which acquire the magnetism. By attentively examining them, I have not yet been able to distinguish, without a trial, which pieces are capable of acquiring magnetism, and which not; the colour, apparent texture, and degree of ductility seeming to afford no sure indication. In short, what I have observed relating to the magnetic properties of brass is:

1st, That most brass becomes magnetic by hammering, and loses the magnetism by annealing or softening in the fire.

2dly, That the acquired magnetism is not owing to particles of iron or steel imparted to the brass by the tools employed.

3dly,

3dly, Those pieces of brass which have that property, retain it without any diminution after a great number of repeated trials, *viz.* after having been repeatedly hardened and softened. But I have not found any means to give that property to such brass as had it not naturally.

4thly, A large piece of brass has generally a magnetic power somewhat stronger than a smaller piece; and the flat surface of the piece draws the needle more forcibly than the edge or corner of it.

5thly, If only one end of a large piece of brass be hammered, then that end alone will disturb the magnetic needle, and not the rest.

6thly, The magnetic power which brass acquires by hammering has a certain limit, beyond which it cannot be increased by farther hammering. This limit is various in pieces of brass of different thickness, and likewise of different quality.

7thly, Though there are some pieces of brass which have not the property, of being rendered magnetic by hammering; yet all the pieces of magnetic brass, that I have tried, lose their magnetism by being made red-hot, excepting indeed when some piece of iron is concealed in them, which sometimes occurs; but in this case, the piece of brass, after having been made red-hot and cooled, will attract the needle more forcibly with one part of its surface than with the rest of it; and hence, by turning the piece of brass about, and presenting every part of it successively to the suspended magnetic needle, one may easily discover in what part of it the iron is lodged.

From those observations it follows, that when brass is to be used for the construction of instruments wherein a magnetic needle is concerned, as dipping needles, variation compasses, &c. the brass should be either left quite soft, or it should be chosen.

chosen of such a sort as will not be made magnetic by hammering, which sort however does not occur very easily.

Examination of the Magnetic Properties of some other Metallic Substances.

The result of the experiments on brass induced me to examine other metallic substances, and especially its components, viz. copper and zinc: though the result of the experiments has not been very remarkable, excepting with platina, which metal has properties in great measure analogous to those of brass.

Having examined various pieces of copper, by means of the suspended magnetic needle, and having never found them magnetic, except only sometimes in such places which had been filed, and where some particles of steel might have been left by the file, I next proceeded to hammer some pieces of it, not only in the usual way, but likewise between flints: the result, however, was very dubious; for though, in general, they had no effect whatever on the needle, yet sometimes I thought the needle was really attracted by some pieces of hammered copper; but then this attractive power was so exceedingly small as not to be depended upon.

Zinc, either not hammered, or hammered as far as could be done without breaking it, shewed no signs of magnetism whatever, when presented to the magnetic needle. A mixture of zinc and tin neither had any action upon the needle.

A piece of a broken reflector of a telescope, which consisted of tin and copper; a mixture of tin, zinc, and a little copper; a piece of silver, both soft and hammered; a piece of pure gold, both soft and hammered; a mixture of gold and silver, both hard and soft; and another mixture of a great deal of
I silver,

silver, a little copper, and a less quantity of gold, either before or after hammering, had not the least action on the magnetic needle,

Platina was the metal I last examined, and the experiments made with it seem to deserve particular attention.

EXPERIMENT I.

A large piece of platina, which, after being precipitated from its solution in *aqua regia*, had been fused, or rather concreted together, being presented to the suspended magnetic needle, shewed not the least sign of magnetism. It was then hammered; but after the third or fourth stroke of the hammer it broke into many pieces, several of which being tried, shewed no magnetism, nor could any of the finest particles be attracted by the magnet presented very nearly over them. The broken surface of this piece of platina was full of cavities, some of which were large, and others just discernible; and altogether the metal seemed to have undergone an imperfect fusion.

EXPERIMENT II.

The grains of native platina were examined next, by putting a magnet just over them; but the magnet attracted not above ten or twenty particles out of about half an ounce of platina: and those which were attracted had either little or no shining metallic appearance like the rest, and were exceedingly small.

EXPERIMENT III.

Having picked out several of the largest grains of platina, I presented the magnet to them; but they were not

in the least attracted by it. One of those grains was then hammered; by which means, after about eight or ten strokes, it was spread into a plate, about a tenth of an inch in diameter, and nearly circular; afterwards the magnet being presented to it, the former attracted it from the distance of about one-twentieth of an inch. The other grains being all hammered one after the other, were rendered by it so far magnetic as to be attracted by the magnet, and to disturb the suspended needle when they were presented to it. But there were some amongst them which acquired no magnetism at all, though they had been purposely hammered much longer than the others.

As far as I could observe, those pieces which would not acquire any magnetism by hammering, had not a very shining appearance before the hammering, though afterwards they could not be distinguished from the others by their appearance; and they seemed not to spread under the hammer so easily as the others.

In general three or four strokes are sufficient to render a grain of platina evidently magnetic, but about ten strokes give it the full power it is susceptible of.

EXPERIMENT IV.

Those grains of platina, which in the preceding experiments had been rendered magnetic by hammering, being put upon a charcoal, were made red-hot by means of a blow-pipe; and afterwards being presented to the magnet, and likewise to the suspended needle, they shewed not the least sign of magnetism. Heat, therefore, deprives them as well as brass of the magnetism acquired by hammering. A second hammering rendered them magnetic, though not so quickly, nor to so great a degree,

degree, as it had done the first time. However, it must be observed, that the pieces of platina having been rendered flat and thin by the first hammering, could not be so easily struck, nor spread much more, by the second.

If it is true, as those experiments seem to prove beyond a doubt, that magnetism may exist, or may belong to other substances, independent of iron, it must follow, that the attraction of a few particles of an unknown substance by the magnet is not a sure sign of the presence of iron. Hence those substances, which hitherto have been considered as containing ferrugineous particles, for no other reason but because the magnet attracted a small quantity of them, must be considered as dubious; and the conclusion of the existence of iron should not be admitted, except when those particles, which have been separated by the magnet, appear to be iron by some other trial; for though it is true, that iron is always attracted by the magnet, yet it does not hence follow, that whatever is attracted by the magnet must be iron.

P O S T S C R I P T.

THE existence of magnetism, or of the power of attracting and being attracted by the magnetic needle, in bodies, without the interference of iron or any ferrugineous matter, being a proposition not only new and singular, but seemingly of importance in philosophy, the experiments which tend to confirm it should be never deemed superfluous, nor any possible objection be left unanswered: hence, since the writing of the foregoing paper, I have endeavoured to raise objections, and to con-

trive means of explaining them; but every consideration seemed to confirm the proposition advanced. The principal of those objections was, that the brass which becomes magnetic by hammering and loses that power by softening, might contain a small quantity of iron, to which that magnetic power was owing; and that this iron or martial earth, dispersed through the substance of the brass, might become phlogisticated by the action of hammering; inasmuch as the brass being forced into a smaller space might perhaps give some of its phlogiston to the martial earth, and thus render it magnetic; and, on the contrary, the action of the fire in softening might remove that phlogiston from the martial earth, and give it to the brass; hence the former, remaining quite dephlogisticated, would no longer shew any signs of magnetism. The consideration that iron may be dephlogisticated or calcined more easily than brass gave an apparent weight to the supposition; but the following experiments seem to expel every doubt.

EXPERIMENT I.

Having chosen a piece of brass which would acquire no magnetism by hammering, I placed it upon the anvil, together with a considerable quantity of *crocus martis*, which crocus had no action on the magnetic needle; then began hammering the brass, and turning it frequently, in order to let part of the crocus adhere to it; and, in fact, the crocus had in several places been fastened so well into the brass, that hard wiping with a woollen cloth would not rub it off. The brass appeared red in those places; but, after having been hammered for a long time, it acquired no magnetism whatever. The hardening, there-

fore, could not render the iron calx so far phlogisticated as to affect the magnetic needle.

EXPERIMENT II.

In order to diversify the preceding experiment, I drilled a hole, about one-eighth of an inch long, and little more than one-fiftieth of an inch in diameter, into a piece of brass that was not rendered magnetic by hammering, and filled it with *crocus martis*; then I hammered the piece of brass, thus inclosing the calx of iron, and afterwards presented it to the needle; but there was not the least sign of attraction: the martial earth, therefore, had not acquired any phlogiston from the brass by the action of hammering.

EXPERIMENT III.

The piece of brass mentioned in the preceding experiment, *viz.* with a little calx of iron in it, was put into the fire, and was made quite red-hot, in which state it remained for about three minutes. Then, after cooling, it was presented to the magnetic needle, and this was attracted by the brass only in that place wherein the calx of iron was contained. The action, therefore, of the fire had rendered the martial earth so far phlogisticated as to attract the magnetic needle; hence, if the magnetism of brass was owing to any ferrugineous matter contained in it, a piece of brass ought to become magnetic when softened, which is contrary to the experiments mentioned in this paper.

EXP B-

EXPERIMENT IV.

A hole, similar to that mentioned in the second experiment, was drilled into a piece of brass that would not become magnetic by hammering, and into it was put some black calx of iron, which was so far phlogisticated as to be attractable by the magnet, and the hole was closed by a few strokes of the hammer. In consequence of which the piece of brass, when presented to the suspended magnetic needle, would attract it only about that place where the magnetic calx was contained. This attraction was very weak. Then the piece of brass, thus prepared, was put into the fire, and was kept for about six minutes, in a heat very little short of that necessary to melt brass, and after cooling I presented it to the needle, expecting that the fire might have dephlogisticated that calx of iron so far as not to let it act any longer upon the needle; but the attraction appeared to be of the same degree it was before the heating.

It seems, therefore, to be demonstrated, as far as the subject will admit of demonstration, that the magnetism acquired by brass, when hammered, is not owing to iron contained in it; and, consequently, that *magnetism, or the power of being attracted by, and attracting, the magnet, may exist independent of iron.*

TO DR. BLAGDEN, SEC. R. S.

S I R,

Windsor, January 9, 1786.

I HAVE made the experiment which you recommended me to try, relating to the magnetism of brass; *viz.* I mixed, by means of the blow-pipe, a small quantity of iron, with about four times its weight of such brass as would not become magnetic by hammering. The whole globule weighed about two grains, and it attracted the magnetic needle very powerfully. I then melted this globule of brass and iron with about fifty grains of the same sort of brass as had been used before. After cooling, the whole lump of brass appeared to have very little power upon the magnetic needle, every part of its surface attracting one end of the suspended needle, so as to let it just adhere to it when the air was not at all disturbed. But this weak and hardly perceivable degree of magnetism was not increased by hammering, nor annihilated by softening.

In the course of my experiments on the magnetism of brass, I have twice observed the following remarkable circumstance. A piece of brass, which had the property of becoming magnetic by hammering, and of losing the magnetism by softening, having been left in the fire till it was partially melted, I found, upon trial, that it had lost the property of becoming magnetic by hammering; but having been afterwards fairly melted in a crucible, it thereby acquired the property it had originally, *viz.* that of becoming magnetic by hammering.

I have

I have likewise often observed, that a long continuance in a fire so strong as to be little short of melting hot, generally diminishes, and sometimes quite destroys, the property of becoming magnetic in brass. At the same time, the texture of the metal is considerably altered, becoming what some workmen call *rotten*. From this it appears, that the property of becoming magnetic in brass by hammering, is rather owing to some particular configuration of its parts, than to the admixture of any iron; which is confirmed still farther by observing, that Dutch plate-brass (which is made not by melting the copper, but by keeping it in a strong degree of heat whilst surrounded by *lapis calaminaris*) also possesses that property; at least all the pieces of it, which I have tried, have that property.

I am, &c.

T. CAVALLO.

IV. *On Infinite Series.* By Edward Waring, M. D. F. R. S.
Lucasian Professor of Mathematics at Cambridge.

Read December 15, 1785.

1. **I**N the Paper, which the Royal Society did me the honour to print, on Summation of Series, is given a method of finding the sum of a series, whose general term $\left(\frac{P}{Q}\right)$ (where $\frac{P}{Q}$ is a fraction reduced to its lowest terms) is a determinate algebraical function of the quantity (z) the distance from the first term of the series, which always terminates when the sum of the series can be expressed in finite terms.

2. The terms of every infinite series must necessarily be given by a function of z , or by quantities which can be reduced to a function of z .

3. Let $Q = A \times A' \times A'' \times \dots \times A^{n-1} \times B \times B' \times B'' \times \dots \times B^{m-1} \times C \times C' \times C'' \times \dots \times C^{r-1} \times \&c.$ where $A', A'', A''' \dots A^{n-1}$, are successive values of A ; that is, result from A by writing in it for z respectively $z+1, z+2, z+3, \dots z+n$; and $B', B'', B''' \dots B^{m-1}$, result from B , by writing in it for z respectively $z+1, z+2, z+3, \dots z+m$; but B is not a successive value of A ; &c. Let the numerator $P = E \cdot E' \cdot E'' \dots E^{b-1} \cdot F \cdot F' \cdot F'' \dots F^{k-1} \cdot L$; $E', E'' \dots E^{b-1}$; $F', F'' \dots F^{k-1}$, &c. denoting successive values of the quantities E, F , &c. respectively; and L , admitting of no divisor of the formula $K \times K'$, where K' is a suc-

cessive value of K : let $L = A \times B \times C \times \&c. \times E^{1b} \times F^{1t} \times \&c. \times p' \times q' \times r' \times \&c. - A^{1a} \times B^{1m} \times C^{1r} \times \&c. \times E \times F \times \&c. \times p \times q \times r \times \&c.$ where $p', q', r', \&c.$ are irrational quantities and successive values of $p, q, r, \&c.$ The factors $A, B, C, \&c. E^{1b}, F^{1t}, \&c.$ being given, the factors $p', q', r', \&c.$ into which they are multiplied in the quantity L will easily be deduced by deducting the preceding irrational factors contained in $A, B, C, \&c. E^{1b}, F^{1t}, \&c.$ from the correspondent irrational factors contained in L ; and in the same manner, from the factors $A^{1a}, B^{1m}, \&c. E, F, \&c.$ can be deduced the irrational factors of the preceding $p, q, r, \&c.$

Assume for the sum of the series sought the quantity

$$\frac{E \times E' \times E^{1a} \dots E^{1b-1} \times F \times F' \dots F^{1t-1} \times \&c.}{A \cdot A' \cdot A^{1a} \dots A^{1a-1} \times B \times B' \times B^{1m-1} \dots B^{1m-1} \times C \times C' \dots C^{1r-1} \times \&c. \times p \times q \times r \times s \times \&c. (ax^m + \beta x^{m-1} + \gamma x^{m-2} + \&c.) = V;}$$

where m is a whole number, and $\alpha, \beta, \gamma, \&c.$ are co-efficients to be investigated; write in V for x its successive value $x+1$, and let the result be W ; reduce the difference $W - V$ into a fraction in its lowest terms, and make the co-efficients of the correspondent terms of the resulting fraction $= (W - V)$ and of the given fraction equal to each other, if possible; and thence may be deduced the sum of the series required.

4. This series will terminate if the sum sought can be expressed by a finite determinate function of x ; if not, it will proceed *in infinitum*, and may be expressed either by a series ascending or descending according to the dimensions of x .

5. If any factor, A or B , or $C, \&c.$ have no successive one in the denominator; or if the greatest dimensions of x in the denominator be greater than its greatest dimensions in the numerator by 1, then the sum of the series is not a finite algebraical function of x .

6. If

6. If in the denominator are deficient some intermediate successive factors, multiply both the numerator and denominator by those deficient factors, and they are supplied: for example, let $A \times A''' \times A'''' \times \&c.$ be factors of the denominator, in which are deficient the factors A' , A'' , A'''' , &c. multiply both numerator and denominator by the content $A' \times A'' \times A'''' \times \&c.$ and they are restored.

7. If in the denominator is contained the content $A' \times A'' \times A''' \dots A'^{\lambda} = H$, in which λ is the least of the indices $l, l', l'', \&c.$: assume $A^{\lambda} \times A'^{\lambda} \times A''^{\lambda} \dots A'^{\lambda} \times \alpha = H$, and in the same manner reduce the factors in α ; then assume for the denominator $A^{\lambda} \times A'^{\lambda} \times A''^{\lambda} \dots A'^{\lambda-1} \times \&c.$: for example, let the contents be $A^3 \times A'^5 \times A''^2 \times A'''^5 \times \&c. = H$, then is 2 the least index, and consequently the content reduced as before taught will be $A^2 \cdot A'^2 \cdot A''^2 \cdot A'''^2 \times \alpha = A^2 \cdot A'^2 \cdot A''^2 \cdot A'''^2 \times A \cdot A' \cdot A'' \times A'^2 \cdot A''^2$; but between the factors A' and A''' is deficient the factor A'' ; and between the factors A'^2 and A'''^2 is deficient the factor A''^2 ; multiply the numerator and denominator of the given fraction by the deficient factors $A'' \times A''^2$; assume for the denominator $A^2 \cdot A'^2 \cdot A''^2 \times A \cdot A' \cdot A'' \times A'^2 \cdot A''^2 \times \&c. = A^3 \cdot A'^5 \cdot A''^5 \times \&c. \&c.$

8. If the greatest index of the content H is contained in one factor only, then the sum of the series cannot be expressed in finite terms of the quantity z .

9. The same may be applied to the contents of the several successive values of the quantities $B, C, \&c.$ in the denominator:

for example, let the general term be $\frac{1}{1 \cdot 2 \cdot 3 \dots z-2 \times z}$; multiply it into $z-1$ to complet the deficient term, and it results

M 2

$\frac{z-1}{1.2.3\dots z}$; assume, by the preceding method for the sum of the series the quantity $\frac{1}{1.2.3\dots z-1}$, of which the successive term is $\frac{1}{1.2.3\dots z}$, and their difference $\frac{1}{1.2\dots z-1} - \frac{1}{1.2\dots z} = \frac{1}{1.2\dots z-2 \times z}$ the given term.

2. Let the term be Ne^z and e less than 1, which is the term of a geometrical series; then will the sum of the infinite series be $\frac{N}{1-e} \times e^z$, beginning from the term whose distance from the first is z ; for the difference between the two successive sums $= \frac{N}{1-e} (e^z - e^{z+1}) = Ne^z$ the given term.

3. Let the general term be $\frac{(z+n+1-e^z \times z+1)^a}{z+1. z+n+1} \times e^{z+1}$; assume for the sum of the series the subsequent quantity $(z+1. z+2. z+3\dots z+n)^{-1} \times e^z \times (a+\beta z+\gamma z^2\dots z^{n-1})$ and by the preceding method the co-efficients a, β, γ , &c. may be found: the sum is known to be $= \frac{a}{z+1} \times e^{z+1} + \frac{\beta}{z+2} \times e^{z+2} + \frac{\gamma}{z+3} \times e^{z+3} \dots \frac{a}{z+n} e^{z+n}$, which can easily be reduced to the preceding formula.

If the general term be $\frac{T'-T}{T \times T'}$ or $\frac{PQ'-QP'}{QQ'}$; where T and T' , P and P' , Q and Q' , are successive terms; then will the sums of the series be $\frac{1}{T}$ or $\frac{P}{Q}$ properly corrected.

10. If the function expressing the general term contain in the denominator a factor or factors, which have no successive one; reduce the factor or factors into an infinite series proceeding according to the dimensions of z , and thence, by the method before given, find the sum of the series. The same method

may be pursued, when the denominator of a fluxion, which is a function of x multiplied into \dot{x} contains the simple power only of a factor or factors; reduce the factor or factors into an infinite series, proceeding according to the dimensions of x , and by the known methods find the fluent of the fluxion.

¶1. The fluent of the fluxion or sum of the series may be deduced also from the subsequent propositions, from which may be investigated many serieses, whose sums are known.

1. Let $p\dot{p} = Q\dot{q}$, $q\dot{Q} = R\dot{r}$, $r\dot{R} = S\dot{s}$, $s\dot{S} = T\dot{t}$, &c.; then $\int P\dot{p} = Pp - Qq + Rr - Ss + Tt - \&c.$ if only the series converges.

2. Let $pP' + p'P = Qq'$, $qQ' + q'Q = Rr'$, $rR' + r'R = Ss'$, $sS' + s'S = Tt'$, &c. where P' , p' , Q' , q' , &c. denote the increments of the quantities P , p , Q , q , &c. respectively, then will the integral of the increment $(P\dot{p}) = Pp - Qq + Rr - Ss + \&c.$ if only the series converges.

Ex. 1. $\int x^{m+n}\dot{x} = \int \frac{x^{m+n}}{x^{n+1}} = \frac{1}{x^{n+1}} \left(\frac{1}{m+1} + \frac{n+m}{r'+1} A + \frac{n+r'}{s'+1} B + \frac{n+s'}{t'+1} C + \frac{n+t'}{u'+1} D + \&c. \right) = \frac{1}{1-n} \frac{1}{x^{n-1}}$; whence $\frac{1}{1-n} = \frac{1}{m+1} + \frac{n+m}{r'+1} \times \frac{1}{m+1} + \frac{n+r'}{s'+1} \times \frac{n+m}{r'+1} \times \frac{1}{m+1} + \&c.$ In this example $p = x^{m+n}$, $P = x^{m+n+1}$, $q = x^{n+1}$, $Q = x^{n+2}$, $r = x^{r'+1}$, &c.

Ex. 2. $\int \frac{x^{m+n}}{x^{n+r}} = \int x^{m-n-r+1} = \frac{1}{m-n-r+1} x^{m-n-r+2} = x^{m-n-r+2} \left(\frac{1}{m+1} + \frac{1}{m+1} \times \frac{n+r}{m+2} + \frac{1}{m+1} \cdot \frac{n+r}{m+2} \cdot \frac{n+r+1}{m+3} + \&c. \right)$; whence $\frac{1}{m-n-r+1} = \frac{1}{m+1} + \frac{n+r}{m+2} A + \frac{n+r+1}{m+3} B + \&c.$ In both these examples the letters A , B , C , &c. denote the preceding terms.

Ex. 3. $\int \frac{x^{m+n}}{1+x} = \frac{x^{m+n}}{1+x} \left(\frac{1}{m+1} + \frac{1}{m+1 \cdot m+2} \times \frac{x}{1+x} + \frac{2}{m+1 \cdot m+2 \cdot m+3} \times \frac{x^2}{(1+x)^2} + \frac{2 \cdot 3}{m+1 \cdot m+2 \cdot m+3 \cdot m+4} \cdot \frac{x^3}{(1+x)^3} + \&c. \right)$

Ex.

$$\text{Ex. 4. } \int \frac{a'x^m}{(a+bx^n+cx^{2n})^b} = \frac{1}{m+1} \frac{a'x^{m+1}}{(a+bx^n+cx^{2n})^b} + \frac{a'x^{m+1}}{m+1} \frac{1}{(a+bx^n+cx^{2n})^{b+1}} \\ \times \left(\frac{nbhx^{n-1}}{m+n+1} + \frac{2nhcx^{2n-1}}{m+2n+1} \right) + \&c.$$

12. Let the general term of an infinite series be

$$\frac{A}{z+\alpha \cdot z+\alpha+1 \cdot z+\alpha+2 \dots z+\alpha+n \times z+\beta \cdot z+\beta+1 \cdot z+\beta+2 \dots z+\beta+m \times z+\gamma \cdot z+\gamma+1 \cdot z+\gamma+2 \dots z+\gamma+r \times z+\delta \cdot z+\delta+1 \cdot z+\delta+2 \dots z+\delta+s \times \&c.}; \text{ where } \alpha-\beta, \alpha-\gamma, \beta-\gamma, \&c. \text{ are not whole numbers; the sum of the series can always be expressed in finite terms; if the sum of the fractions } \frac{1}{1 \cdot 2 \cdot 3 \dots n} \times \frac{\beta-\alpha \cdot \beta-\alpha+1 \cdot \beta-\alpha+2 \dots \beta-\alpha+m \times \gamma-\alpha \cdot \gamma-\alpha+1 \cdot \gamma-\alpha+2 \dots \gamma-\alpha+r}{\times \delta-\alpha \cdot \delta-\alpha+1 \cdot \delta-\alpha+2 \dots \delta-\alpha+s \times \&c.} \times \frac{1}{1 \times 1 \cdot 2 \cdot 3 \dots n-1} \times \frac{\beta-\alpha-1 \cdot \beta-\alpha \cdot \beta-\alpha+1 \cdot \beta-\alpha+2 \dots \beta-\alpha+m-1 \times \gamma-\alpha-1 \cdot \gamma-\alpha \cdot \gamma-\alpha+1 \dots \gamma-\alpha+r-1 \times \delta-\alpha-1 \cdot \delta-\alpha \cdot \delta-\alpha+1 \dots \delta-\alpha+s-1 \times \&c.} + \frac{1}{1 \cdot 2 \times 1 \cdot 2 \cdot 3 \dots n-2} \times \frac{\beta-\alpha-2 \cdot \beta-\alpha-1 \cdot \beta-\alpha \cdot \beta-\alpha+1 \dots \beta-\alpha+n-2 \times \gamma-\alpha-2 \cdot \gamma-\alpha-1 \cdot \gamma-\alpha \dots \gamma-\alpha+n-2 \times \delta-\alpha-2 \cdot \delta-\alpha-1 \cdot \delta-\alpha \dots \delta-\alpha+n-2 \times \&c.} \times \frac{1}{1 \cdot 2 \cdot 3 \times 1 \cdot 2 \dots n-3} \times \frac{\beta-\alpha-3 \cdot \beta-\alpha-2 \dots \beta-\alpha+n-3 \times \gamma-\alpha-3 \cdot \gamma-\alpha-2 \dots \gamma-\alpha+n-3 \times \delta-\alpha-3 \cdot \delta-\alpha-2 \dots \delta-\alpha+s-3 \times \&c.} + \dots \frac{1}{1 \cdot 2 \cdot 3 \dots n} \times \frac{\beta-\alpha-n \cdot \beta-\alpha-n+1 \dots \beta-\alpha-n+m \times \gamma-\alpha-n \cdot \gamma-\alpha-n+1 \dots \gamma-\alpha-n+r \times \delta-\alpha-n \cdot \delta-\alpha-n+1 \dots \delta-\alpha-n+s \times \&c.} = 0; \frac{1}{1 \cdot 2 \cdot 3 \dots m} \times \frac{\alpha-\beta \cdot \alpha-\beta+1 \dots \alpha-\beta+m \times \gamma-\beta \cdot \gamma-\beta+1 \dots \gamma-\beta+r \times \delta-\beta \cdot \delta-\beta+1 \dots \delta-\beta+s \times \&c.} \times \frac{1}{1 \times 1 \cdot 2 \cdot 3 \dots m-1} \times \frac{\alpha-\beta-1 \cdot \alpha-\beta \dots \alpha-\beta+m-1}{\dots}$$

$$\begin{aligned}
 & \frac{1}{1 \cdot 2 \times \gamma - \beta - 1 \cdot \gamma - \beta \dots \gamma - \beta + r - 1 \times \delta - \beta - 1 \cdot \delta - \beta \dots \delta - \beta + s - 1 \times \&c.} + \\
 & \frac{1}{1 \cdot 2 \times 1 \cdot 2 \cdot 3 \dots m - 2} \times \frac{1}{\alpha - \beta - 2 \cdot \alpha - \beta - 1 \cdot \alpha - \beta \dots \alpha - \beta + n - 2 \times \gamma - \beta - 2} \\
 & \frac{1}{1 \cdot 2 \cdot 3 \times 1 \cdot 2 \cdot 3 \dots m - 3} \times \frac{1}{\alpha - \beta - 3 \times \alpha - \beta - 2 \times \alpha - \beta - 1 \dots \alpha - \beta + n - 3 \times \gamma - \beta - 3} \\
 & \frac{1}{1 \cdot 2 \cdot 3 \cdot 4 \dots m} \times \frac{1}{\alpha - \beta - m \cdot \alpha - \beta - m + 1 \dots \alpha - \beta + n - m \times \gamma - \beta - m \cdot \gamma - \beta - m + 1 \dots \gamma - \beta - m + r \times \delta - \beta - m \cdot \delta - \beta - m + 1 \dots \delta - \beta - m + s \times \&c.} = 0; \\
 & \frac{1}{1 \cdot 2 \cdot 3 \dots r} \times \frac{1}{\alpha - \gamma \cdot \alpha - \gamma + 1 \dots \alpha - \gamma + n \times \beta - \gamma \cdot \beta - \gamma + 1 \cdot \beta - \gamma + m} \\
 & \times \delta - \gamma \cdot \delta - \gamma + 1 \dots \delta - \gamma + s \times \&c. - \&c. = 0, \&c. = 0; \text{ or, to explain}
 \end{aligned}$$

it otherwise, assume the quantities $\alpha, \alpha + 1, \alpha + 2, \alpha + 3, \dots \alpha + n$; $\beta, \beta + 1, \beta + 2, \dots \beta + m$; $\gamma, \gamma + 1, \gamma + 2, \dots \gamma + r$; $\delta, \delta + 1, \delta + 2, \dots \delta + s$; &c. subtract one α from all the remaining quantities $\alpha + 1, \alpha + 2, \dots \alpha + n$; $\beta, \beta + 1, \dots \beta + m$; $\gamma, \gamma + 1, \dots \gamma + r$; $\delta, \delta + 1, \dots \delta + s$; &c. and multiply all the differences resulting $1, 2, 3, \dots n$; $\beta - \alpha, \beta - \alpha + 1, \dots \beta - \alpha + m$; $\gamma - \alpha, \gamma - \alpha + 1, \dots \gamma - \alpha + r$; $\delta - \alpha, \delta - \alpha + 1, \dots \delta - \alpha + s$, &c. into each other, and call the content p . In the same manner subtract $\alpha + 1$ from all the remaining quantities $\alpha, \alpha + 2, \alpha + 3, \dots \alpha + n$; $\beta, \beta + 1, \dots \beta + m$; $\gamma, \gamma + 1, \dots \gamma + r$; $\delta, \delta + 1, \dots \delta + s$; &c.; and let the remainders $-1, 1, 2, 3, \dots n - 1$; $\beta - \alpha - 1, \beta - \alpha, \beta - \alpha + 1, \dots \beta - \alpha + m - 1$; $\gamma - \alpha - 1, \gamma - \alpha, \gamma - \alpha + 1, \dots \gamma - \alpha + r - 1$; $\delta - \alpha - 1, \delta - \alpha, \delta - \alpha + 1, \dots \delta - \alpha + s - 1$, &c. be multiplied together, and their content be called p^{a+1} . In the same manner subtract $\alpha + 2, \alpha + 3, \dots \alpha + n$ respectively from all the remaining quantities, and let the differences resulting

suming be multiplied together, and their respective contents be called $p^{+2}, p^{+3}, p^{+4}, \dots p^{+n}$; then, if the sum of the series can be found, will $\frac{1}{p^2} + \frac{1}{p^2+1} + \frac{1}{p^2+2} + \frac{1}{p^2+3} \dots \frac{1}{p^2+n} = 0$.

In the same manner subtract $\beta, \beta+1, \beta+2, \dots \beta+m$ respectively from all the remaining quantities, and multiply their respective remainders into each other, and call their contents respectively $p^\beta, p^{\beta+1}, p^{\beta+2}, \dots p^{\beta+m}$, then will $\frac{1}{p^\beta} + \frac{1}{p^{\beta+1}} + \frac{1}{p^{\beta+2}} \dots \frac{1}{p^{\beta+m}} = 0$.

Subtract $\gamma, \gamma+1, \gamma+2, \dots \gamma+r$ respectively from all the remaining quantities, and multiply their respective remainders into each other, and call their contents respectively $p^\gamma, p^{\gamma+1}, p^{\gamma+2}, \dots p^{\gamma+r}$; then will $\frac{1}{p^\gamma} + \frac{1}{p^{\gamma+1}} + \frac{1}{p^{\gamma+2}} \dots \frac{1}{p^{\gamma+r}} = 0$.

Subtract $\delta, \delta+1, \delta+2, \dots \delta+s$ from all the remaining quantities, and multiply their respective remainders into each other, and call their contents respectively $p^\delta, p^{\delta+1}, p^{\delta+2}, p^{\delta+3}$; then will $\frac{1}{p^\delta} + \frac{1}{p^{\delta+1}} + \frac{1}{p^{\delta+2}} + \frac{1}{p^{\delta+3}} = 0$; and so on; and, *vice versa*, if the sum of the above-mentioned fractions be respectively $= 0$; then the sum of the series, whose general term is the given one, can be found; otherwise not.

13. If the sum of the series, whose general term is

$$\frac{1}{x+a \cdot x+a+1 \dots x+a+n \times x+\beta \cdot x+\beta+1 \dots x+\beta+m \times x+\gamma \cdot x+\gamma+1 \dots$$

$x+\gamma+r \times x+\delta \cdot x+\delta+1 \dots x+\delta+s \times \&c. = H$, can be found in finite terms; then the sum of a series, whose general term is $\frac{ax^l + bx^{l-1} + cx^{l-2} + \&c.}{H}$ (where l denotes an affirmative number)

can be also expressed in finite terms.

14. Let

14. Let $A \times \overline{z+\beta} \cdot \overline{z+\gamma} \cdot \overline{z+\delta} \times \overline{z+\epsilon} \times \&c. + B \times \overline{z+\alpha} \times \overline{z+\gamma} \cdot \overline{z+\delta} \cdot \overline{z+\epsilon} \cdot \&c. + C \times \overline{z+\alpha} \cdot \overline{z+\beta} \times \overline{z+\delta} \cdot \overline{z+\epsilon} \cdot \&c. + D \times \overline{z+\alpha} \cdot \overline{z+\beta} \cdot \overline{z+\gamma} \times \overline{z+\epsilon} \cdot \&c. + E \times \overline{z+\alpha} \cdot \overline{z+\beta} \cdot \overline{z+\gamma} \cdot \overline{z+\delta} \times \&c. = 1$, whatever may be the value of z ; then will

$$A = \frac{1}{\beta-\alpha \cdot \gamma-\alpha \cdot \delta-\alpha \cdot \epsilon \cdot \&c.}, B = \frac{1}{\alpha-\beta \cdot \gamma-\beta \cdot \delta-\beta \cdot \epsilon \cdot \&c.}, C = \frac{1}{\alpha-\gamma \cdot \beta-\gamma \cdot \delta-\gamma \cdot \epsilon \cdot \&c.},$$

$$D = \frac{1}{\alpha-\delta \cdot \beta-\delta \cdot \gamma-\delta \cdot \epsilon \cdot \&c.}, \&c.$$

15. Let the general term of a series be $\frac{1}{H} \times e^u$, where e is less than 1; the sum of the series can be expressed in finite

terms, when the above-mentioned quantities $\frac{1}{p\alpha} + \frac{1}{p\alpha+1} + \frac{1}{p\alpha+2}$

$$+ \frac{1}{p\alpha+3} \cdot \&c. = 0, \frac{1}{p\beta} + \frac{1}{p\beta+1} + \frac{1}{p\beta+2} + \frac{1}{p\beta+3} \cdot \&c. = 0,$$

$$\frac{1}{p\gamma} + \frac{1}{p\gamma+1} + \frac{1}{p\gamma+2} \cdot \&c. = 0, \frac{1}{p\delta} + \frac{1}{p\delta+1} + \frac{1}{p\delta+2} + \frac{1}{p\delta+3} \cdot \&c.$$

$$\frac{1}{p\epsilon} = 0, \&c. = 0. \text{ This never happens unless } e = 1.$$

16. If the general term be any rational function of z into the exponential e^u , viz. $\frac{az^l + bz^{l-1} + cz^{l-2} + \&c.}{z+\alpha \times z+\alpha+\beta \times z+\alpha+\gamma \times \&c. \times z+\beta' \times$

$$\frac{\&c.}{z+\beta+b' \times z+\beta+k' \times \&c. \times z+\gamma \times z+\gamma+b'' \times z+\gamma+k'' \times \&c.} \times e^u = K,$$

where $b, b', b'', \&c. k, k', k'', \&c. l, m, m', m'', \&c. n, n', n'', \&c. r, r', r'', \&c. \&c.$ denote whole numbers, and neither

$\alpha = \beta$, nor $\alpha = \gamma$, nor $\beta = \gamma$, &c. are whole numbers: let m be the greatest of the indices $m, m', m'',$ &c.; n the greatest of the indices $n, n', n'',$ &c.; r the greatest of the indices $r, r', r'',$ &c.; then, from the sums of the series given, whose general terms are $\frac{e^x}{(z+\alpha)^m}, \frac{e^x}{(z+\alpha)^{m-1}}, \frac{e^x}{(z+\alpha)^{m-2}}, \dots, \frac{e^x}{(z+\alpha)^2}, \frac{e^x}{z+\alpha};$
 $\frac{e^x}{(z+\beta)^n}, \frac{e^x}{(z+\beta)^{n-1}}, \dots, \frac{e^x}{z+\beta}; \frac{e^x}{(z+\gamma)^r}, \frac{e^x}{(z+\gamma)^{r-1}}, \dots, \frac{e^x}{z+\gamma},$ &c. can be deduced the sum of the above series, whose general term is given above; multiply each of these terms into unknown coefficients $e', f, g, h,$ &c.; then reduce them to a common denominator, which is the same as the denominator of the given general term, and add them together, and make the correspondent terms of the sum resulting equal to the correspondent terms of the numerator $ax^l + bx^{l-1} + cx^{l-2} +$ &c. of the given general term; and from the equations resulting can be deduced the co-efficients $e', f, g, h,$ &c. and thence from the given sums the sum of the series required.

Approximations to the sums of the series may be deduced from the methods given in the *Meditationes Analyticae*. The sum of some few cases have been given from the periphery of the circle: for example, when α and m are whole numbers, and $e = 1$; or, more particularly, when $m = 2$ and $e = \frac{1}{2}$; and some other particular cases, which may be with nearly the same facility calculated from approximations; the cases given indeed are so few, unless when $e = 1$, that they can very rarely be applied.

17. If the dimensions of z in the numerator be equal or greater than its dimensions in the denominator; that is, l be equal or greater than $m + m' + m'' +$ &c. $+ n + n' + n'' +$ &c. $+ r + r' + r'' +$

+ &c. &c. reduce the fractions to a mixed number, so that the dimensions of z in the numerator of the fraction be less than its dimensions in the denominator, and the integral part be $Ae^z + Bze^z + Cz^2e^z + Dz^3e^z + \dots Hze^z$: the sum of the infinite series whose term is $Ae^z + Bze^z + Cz^2e^z + Dz^3e^z + Ez^4e^z + \dots$

+ $Hze^z = \frac{Ae}{1-e} + B\left(\frac{1}{(1-e)^2} - \frac{1}{1-e}\right) + C\left(\frac{1 \cdot 2}{(1-e)^3} - \frac{1}{(1-e)^2}\right) + De\left(\frac{1 \cdot 2 \cdot 3}{(1-e)^4} - \frac{1+2 \times 1 \cdot 2}{(1-e)^3} + \frac{1}{(1-e)^2}\right) + Ee\left(\frac{1 \cdot 2 \cdot 3 \cdot 4}{(1-e)^5} - \&c.\right)$: the sum of a series (whose general term is $z^m e^z = \frac{1 \cdot 2 \cdot 3 \cdot 4 \dots m}{(1-e)^{m+1}} e -$

$\frac{1^{m-1}}{(1-e)^m} 1 \cdot 2 \cdot 3 \dots m-1 e + \frac{1^{m-1} \times 1^{m-2} \dots 1^{m-1}}{(1-e)^{m-1}} \times 1 \cdot 2 \cdot 3 \dots m-2 \cdot e$

$\dots + \frac{L}{(1-e)^{m-b}} \times 1 \cdot 2 \cdot 3 \dots m-b-1 \dots \pm \frac{1}{(1-e)^2}$, where L is

equal to the sum of all quantities of the following sort, ${}^1S^{m-1} \times {}^2S^{m-a-1} \times {}^3S^{m-a-\beta-1} \times {}^4S^{m-a-\beta-\gamma-1} \times {}^5S^{m-a-\beta-\gamma-\delta-1} \times \&c.$

where $\alpha, \beta, \gamma, \delta, \&c.$ are whole affirmative numbers; (in the preceding notation by 1S is designed the sum of the contents of every π of the following numbers $1, 2, 3, 4, 5, \dots, p$) and $\alpha + \beta + \gamma + \delta + \dots = b + 1$; the above-mentioned product ${}^1S^{m-1} \times {}^2S^{m-a-1} \times \&c.$ is to be taken affirmative or negative, according as the number of letters $\alpha, \beta, \gamma, \delta, \&c.$ is even or uneven.

The sum of the series $z^m \times e^z$ may also be found by assuming for it $(az^m + bz^{m-1} + cz^{m-2} + dz^{m-3} \dots k) e^z$; then, finding its successive term $(a \times z + 1 e + bz + 1 e + cz + 1 e + \&c.) e^z$, and taking the difference between it and the assumed quantity, there results $(a \times e - 1 z^m + mae + be - 1 z^{m-1} + \&c.) e^z$; by equating it to the given term $z^m e^z$ are deduced the subsequent equations $a \times e - 1 = 1, mae + be - 1 = 0, \&c.$ whence

N 2

a =

$$a = \frac{1}{e-1}, \quad b = \frac{1}{e} - ma, \quad \&c.$$

If $e = 1$, then assume $ax^{m+1} + bx^m + \&c.$ for the sum sought, which rule was first taught by M. J. BERNOULLI.

If the term be $x^m f^{bn}$; for f^b substitute e , and there results $x^m e^n$ the same as before.

18. Let $P = A + Bx^n + Cx^{2n} + Dx^{3n} + \&c.$ then will the sum of the series $\frac{A}{a \cdot \beta \cdot \gamma \cdot \delta \cdot \&c.} + \frac{Bx^n}{a+n \cdot \beta+n \cdot \gamma+n \cdot \delta \cdot \&c.} +$

$$\frac{Cx^{2n}}{a+2n \cdot \beta+2n \cdot \gamma+2n \cdot \delta \cdot \&c.} + \&c. = \frac{1}{a\beta\gamma\delta \cdot \&c.} \times P - \frac{1}{a} \cdot \frac{1}{\beta-a} \cdot \frac{1}{\gamma-a} \cdot \frac{1}{\delta-a} \cdot \&c. \times \&c. x^n \int x^n p - \frac{1}{\beta} \cdot \frac{1}{a-\beta} \cdot \frac{1}{\gamma-\beta} \cdot \frac{1}{\delta-\beta} \cdot \&c. x^{2n} \int x^{2n} p - \frac{1}{\gamma} \cdot \frac{1}{a-\gamma} \cdot \frac{1}{\beta-\gamma} \cdot \frac{1}{\delta-\gamma} \cdot \&c. \times x^{3n} \int x^{3n} p - \&c.$$

This may be proved from the subsequent arithmetical proposition $\frac{1}{a} \cdot \frac{1}{\beta-a} \cdot \frac{1}{\gamma-a} \cdot \frac{1}{\delta-a} \cdot \&c. + \frac{1}{\beta} \cdot \frac{1}{a-\beta} \cdot \frac{1}{\gamma-\beta} \cdot \frac{1}{\delta-\beta} \cdot \&c. + \frac{1}{\gamma} \cdot \frac{1}{a-\gamma} \cdot \frac{1}{\beta-\gamma} \cdot \frac{1}{\delta-\gamma} \cdot \&c. + \frac{1}{\delta} \cdot \frac{1}{a-\delta} \cdot \frac{1}{\beta-\delta} \cdot \frac{1}{\gamma-\delta} \cdot \&c. = \frac{1}{a \cdot \beta \cdot \gamma \cdot \delta \cdot \&c.}$

19. Let the general term of the above-mentioned series $A + Bx^n + Cx^{2n} + \&c.$ be Hx^{ln} ; then from the sums of the series p , and the fluents of the fluxions $x^n p$, $x^{2n} p$, $x^{3n} p$, $x^{4n} p$, $\&c.$ being given, there follows the sum of a series, whose general term is $\frac{ax^{ln} + bx^{ln-1} + cx^{ln-2} + \&c.}{a+n \cdot \beta+n \cdot \gamma+n \cdot \delta+n \cdot \&c.} \times Hx^{ln}$, where l denotes a whole number.

If $H = m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \dots \cdot \frac{m-2}{z+1}$, or $m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \dots \cdot \frac{m-l}{lz+1}$, where l is a whole number, and $m, \alpha', \beta', \gamma', \&c.$ are either whole numbers or fractions whose denominator is 2, and $\alpha = \alpha'n, \beta = \beta'n, \&c.$ the sum of the above-mentioned series can be found by finite terms, circular arcs and logarithms. If

If $H = \frac{1}{1 \cdot 2 \cdot 3 \dots x}$ or $= \frac{1}{1 \cdot 2 \cdot 3 \dots lx}$, and l, a, β, γ , &c. whole numbers and $n = 1$; then can the sum of any series of the above-mentioned formula be found in finite algebraical functions of x , and the circular arcs and logarithms of them.

P A R T II

1. THE doctrine of proportional parts was probably very early known in the æra of science; for when men could not find the exact value of a quantity, they were induced to find near approximations by trials, and from thence, by proportion, an approximation still nearer: which method is commonly denominated the Rule of False.

This was often found to deviate considerably from the exact value; and the same operation was repeated, which frequently produced a nearer approximate value, and so on.

This method of approximations, the most general yet known, has been used in resolving problems by several of the most eminent mathematicians in different ages, and in this particularly by M. EULER.

2. The following observation, I believe, was first published in the *Meditationes*, in the year 1770, viz. that the convergency of the approximate values, found by the rule of false and method of infinite series, generally depended on this, viz. how.

how much nearer the approximate assumed is to one value of the quantity sought possible or impossible than to any other, and not to the quantity itself: hence, when two or more (n) values of the quantity sought are nearly equal, it is necessary to recur to more difficult rules, *viz.* to three or more trials; as, for example, let two roots be nearly equal, and write a , $a + \pi$, and $a + \rho$, for the unknown quantity in the given equation made $= 0$, and let the quantities resulting be A , B , and C , then will more near approximations to the two roots nearly equal of the given equation be $a +$ the two roots (x) of the quadratic $\left(\frac{A}{\pi\rho} + \frac{B}{\pi(\pi-\rho)} + \frac{C}{\rho(\rho-\pi)}\right)x^2 - \left(A \times \frac{\pi+\rho}{\pi\rho} + B \times \frac{\rho}{\pi \cdot (\pi-\rho)} + \frac{C\pi}{\rho \cdot (\rho-\pi)}\right)x + A = 0$: for write o , π , and ρ , respectively for x in the equation, and there will result the quantities A , B , and C .

More generally, substitute for x in the given equation the quantities a , $a + \pi$, $a + \rho$, $a + \sigma$, $a + \tau$, $a + \nu$, &c. where π , ρ , σ , τ , &c. are very small quantities; and let the quantities resulting be A , B , C , D , E , F , &c.; then will more near approximations to the (n) roots of the given equation be $a + \alpha$, $a + \beta$, $a + \gamma$, $a + \delta$, &c. where α , β , γ , δ , &c. are the n roots (e) of the given equation $\frac{(e-\pi)(e-\rho)((e-\sigma)(e-\tau)&c.)}{\pm \pi\rho\sigma\tau, \&c.} \times A + \frac{e(e-\rho)(e-\sigma)(e-\tau)&c.}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\tau)&c.} \times B + \frac{e(e-\pi)(e-\sigma)(e-\tau)&c.}{\rho(e-\pi)(e-\sigma)(e-\tau)&c.} \times C + \&c. = A - \left(\frac{A}{\pi} - \frac{B}{\pi}\right)e + \left(\frac{A}{\pi\rho} + \frac{B}{\pi(\pi-\rho)} + \frac{C}{\rho(\rho-\pi)}\right) \times e \cdot \overline{e-\pi} - \left(\frac{A}{\pi\rho\sigma} - \frac{B}{\pi(\pi-\rho)(\pi-\sigma)} - \frac{C}{\rho(\rho-\pi)(\rho-\sigma)} - \frac{D}{\sigma(\sigma-\pi)(\sigma-\rho)}\right) \times e \cdot \overline{e-\pi} \cdot \overline{e-\rho} + \left(\frac{A}{\pi\rho\sigma\tau} + \frac{B}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\tau)} + \frac{C}{\rho(\rho-\pi)(\rho-\sigma)(\rho-\tau)} + \frac{D}{\sigma(\sigma-\pi)(\sigma-\rho)(\sigma-\tau)} + \frac{E}{\tau(\tau-\pi)(\tau-\rho)(\tau-\sigma)} + \frac{F}{\nu(\nu-\pi)(\nu-\rho)(\nu-\sigma)(\nu-\tau)}\right) \times e \cdot \overline{e-\pi} \cdot \overline{e-\rho} \cdot \overline{e-\sigma} - \left(\frac{A}{\pi\rho\sigma\nu} - \frac{B}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\nu)} - \frac{C}{\rho(\rho-\pi)(\rho-\sigma)(\rho-\nu)} - \frac{D}{\sigma(\sigma-\pi)(\sigma-\rho)(\sigma-\nu)} - \frac{E}{\tau(\tau-\pi)(\tau-\rho)(\tau-\sigma)(\tau-\nu)} - \frac{F}{\nu(\nu-\pi)(\nu-\rho)(\nu-\sigma)(\nu-\tau)}\right) \times e \cdot \overline{e-\pi} \cdot \overline{e-\rho} \cdot \overline{e-\sigma} \cdot \overline{e-\tau} + \&c.$

$$\frac{(\epsilon - \tau)(\epsilon - \nu)}{\sigma(\sigma - \pi)(\sigma - \epsilon)(\sigma - \tau)(\sigma - \nu)} - \frac{D}{\tau(\tau - \pi)(\tau - \epsilon)(\tau - \sigma)(\tau - \nu)} - \frac{E}{\nu(\nu - \pi)(\nu - \epsilon)(\nu - \sigma)(\nu - \tau)} = 0.$$

Their resolutions were first given in the *Meditationes Analyticae*, published in the year 1774, and require the extraction of a quadratic, cubic, and in general of an equation of (n) dimensions; which rules will often give a nearer approximate than the preceding, when the roots are not nearly equal.

3. These rules may be applied to find approximations to the roots of algebraical equations: for example, let the algebraical equation be $x^n - px^{n-1} + qx^{n-2} - \&c. = 0$, substitute in it for x two quantities a , and $a + e$ much nearer to one root than to any other, and there result $a^n - pa^{n-1} + qa^{n-2} - \&c. = A$, and $(a + e)^n - p(a + e)^{n-1} + q(a + e)^{n-2} - \&c. = B$; then, by the rule of false, $B - A : e :: A : \frac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{(na^{n-1} - n - 1pa^{n-2} + n - 2qa^{n-3} - \&c.) + \&c.} = b$;

whence $a + b$ a near approximate value to the root sought. If the quantities, in which are involved $e, e^2, e^3, \&c.$ on account of e being very small, be rejected, then will the approximate sought $b =$

$$\frac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{na^{n-1} - n - 1pa^{n-2} + n - 2qa^{n-3} - \&c.};$$

which will nearly be the same as found, where a near approximate is given, from the method given by VIETA, HARRIOT, OUGHTRED, NEWTON, DE LAGNY, HALLEY, &c..

4. From this expression it follows, that if (a) be a root of the equation $na^{n-1} - n - 1pa^{n-2} + \&c. = 0$, of which the roots are limits between the roots of the given equation, the approximation found will be infinite.

5. In finding these approximations, when there are two or more quantities contained in the given equation dependent on each.

each other, as the arc and the sine, it is necessary that both should be corrected in every approximation to such a degree as the subsequent approximations require.

6. In the *Meditationes* it is observed, that in any algebraical equation $x^n - ax^{n-1} + bx^{n-2} - cx^{n-3} + dx^{n-4} - ex^{n-5} \dots \pm gx^{n-m+1} \mp bx^{n-m} \pm kx^{n-m-1} \mp lx^{n-m-2} \pm \&c. = 0$, if a be much greater than $\frac{b}{a}$, and $\frac{b}{a}$ has to $\frac{c}{b}$ a much greater ratio than $a : \frac{b}{a}$; and in the same manner $\frac{c}{b}$ has to $\frac{d}{c}$ a much greater ratio than $\frac{b}{a} : \frac{c}{b}$, and so on; then will a be a near approximate to the greatest root of the algebraical equation; $\frac{b}{a}$ a near approximate to the second; $\frac{c}{b}$ a near approximate to the third, and $\frac{d}{b}$ a near approximate to a root, which is much less than m roots of the given equation, but much greater than the remaining $(n - m - 1)$ roots.

If the equation above-mentioned $x^n \pm ax^{n-1} + \&c. = 0$, or which is the same, $1 \pm \frac{a}{x} + \frac{b}{x^2} \pm \&c. = 0$ be infinite; then will, in like manner, all its roots be possible and their approximate values a , $\frac{b}{a}$, $\frac{c}{b}$, $\&c.$ as before.

This easily appears by substituting for a , b , c , $\&c.$ their values in terms of the root of the equation.

7. A nearer approximate to the above-mentioned root will be $\frac{k}{b} - \left(\frac{1}{k} - \frac{gk^2}{b^3} \right) + \&c.$

8. Equations, of which the fluxions of the quantities contained in the given equations can be found; may be reduced to infinite algebraical equations, in which is involved no irrational function of the unknown quantities contained in the given equations by the incremental theorem; viz. let $A = 0$ be the given equation,

equation, and (a) an approximate much more near to the root required (π) of the given equation than to any other: write a for (x) the unknown quantity sought in the subsequent quantities, A , $\frac{\dot{A}}{x}$, $\frac{\ddot{A}}{x^2}$, $\frac{\ddot{\ddot{A}}}{x^3}$, &c.; and let there result the correspondent quantities α , β , γ , δ , &c.; then will $\pi - a$ be a root (e) of the infinite equation $\alpha \pm \beta e + \frac{1}{1 \cdot 2} \gamma e^2 \pm \frac{1}{1 \cdot 2 \cdot 3} \delta e^3 + \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} \epsilon e^4 \pm \&c. = 0$, of which a root of the equations $\alpha \pm \beta e = 0$, $\alpha \pm \beta e + \frac{1}{1 \cdot 2} \gamma e^2 = 0$, &c. will be an approximate. If two roots of the given equation are nearly $= a$, then it is necessary to recur to an equation not inferior to a quadratic.

9. The successive approximate values found by these and like rules will ultimately be to each other in a greater than any geometrical ratio: for example, let $\frac{as}{a+s}$ be an approximate to a root of the quadratic $x^2 - (a+s)x + as = 0$, then will the new addition to the approximate to the root s found by the common method at the distance $n-1$ from the first approximate be $\frac{s^b}{s^{n-1}}$ nearly, where $b = 2^{n-1}$.

10. Let an equation $x^n - Px^{n-1} + Qx^{n-2} - Rx^{n-3} + Sx^{n-4} - \&c. = 0$, of which the roots are a, b, c, d , &c.; and an equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} - \&c. = 0$, where p, q, r, s , &c. differ from the co-efficients P, Q, R, S , &c. by very small quantities: assume the (n) equations $\pi + \rho + \sigma + \tau + \&c. = p - P = \alpha$, $a\pi + b\rho + c\sigma + d\tau + \&c. = Pa - q + Q = \beta$, $a^2\pi + b^2\rho + c^2\sigma + d^2\tau + \&c. = P\beta - Q\alpha + r - R = \gamma$, $a^3\pi + b^3\rho + c^3\sigma + d^3\tau + \&c. = P\gamma - Q\beta + R\alpha - s + S = \delta$, &c.; and from them find the unknown quantities π, ρ, σ, τ , &c.; then will $a + \pi, b + \rho, c + \sigma, d + \tau$, &c. be nearly the n roots of the equation $x^n - px^{n-1} + qx^{n-2} - \&c. = 0$.

$- \&c. = c$, and consequently $\pi, \rho, \&c.$ nearly $-\frac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{na^{n-1} - n - 1pb^{n-2} + \&c.}$

$\frac{b^n + pb^{n-1} + qb^{n-2} - \&c.}{nb^{n-1} - n - 1pb^{n-2} + \&c.}$, $\&c.$; whence the convergency of the ap-

proximate values found by this rule depends on the principle before delivered.

10. Let there be given (m) equations, which contain m unknown quantities $x, y, z, \&c.$; and let $a, \beta, \gamma, \&c.$ be nearly correspondent values of the unknown quantities $x, y, \&c.$ respectively: assume $n + 1$ different values of the quantity x , viz. $a, a + \pi, a + \pi', a + \pi'', \&c. \&c.$; and in like manner assume $n + 1$ different correspondent values of the quantity y , which let be $\beta, \beta + \rho, \beta + \rho', \beta + \rho'', \&c.$; and so of the remaining; where $\pi, \pi', \pi'', \&c. \rho, \rho', \rho'', \&c. \&c.$ are very small quantities; substitute these quantities for their respective values in the given equations, and let the resulting quantities be $A, B, C, D, \&c.$ in the first equation; $P, Q, R, S, \&c.$ in the second, $\&c. \&c.$: assume from the first equation the n simple equations $a\pi + b\rho + \&c. = B - A, a\pi' + b\rho' + \&c. = C - A, a\pi'' + b\rho'' + \&c. = D - A, \&c.$; and from the second equation the n simple ones $b\pi + k\rho + \&c. = Q - P, b\pi' + k\rho' + \&c. = R - P, b\pi'' + k\rho'' + \&c. = S - P, \&c.$ From these equations can be investigated the co-efficients $a, b, \&c. h, k, \&c. \&c.$; ultimately assume the m equations $A + ae + bi + \&c. = 0, P + be + ki + \&c. = 0, \&c.$ from which can be deduced the values of the quantities $e, i, \&c.$; and $a + e, \beta + i, \&c.$ will be more near values of the quantities, $x, y, \&c.$

11. Sir ISAAC NEWTON found the sum (A) of the $2n^{\text{th}}$ power of each of the roots of a given equation, and then extracted the $2n^{\text{th}}$ root of A , viz. $\sqrt[2n]{A}$ for an approximate value of the greatest root of the equation, and further added some similar rules on the same principle. In

In the *Miscell. Analyt.* and *Meditationes* the same principle is applied in different rules for finding approximates to the greatest and other roots of the given equation; and also limits of the ratios of the approximate values of the roots found by these rules to the roots themselves are given.

It is observed in the *Meditationes*, that from these rules in general to find the greatest root, it is often necessary that the greatest possible root be greater than the sum of the quantities contained in the possible and impossible part of any impossible root of the given equation: for example, if $a + b\sqrt{-1}$ be an impossible root of the given equation, then it is necessary that the greatest possible root be greater than $a + b$.

It may further be observed, that in equations of high dimensions (unless purposely made) it is probable, the number of impossible will greatly exceed the number of possible roots; and consequently these rules most commonly fail.

12. M. BERNOULLI assumed a fraction whose numerator is a rational function of the unknown quantity, and denominator the quantity, which constitutes the equation; and reduced the fraction into a series, whose terms proceed according to the dimensions of the unknown quantity; and thence found an approximate value of the greatest or least root of the given equation or its reciprocal, by dividing the co-efficient of any term of the series resulting by the co-efficient of the preceding or subsequent term: for example, let the equation be $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} \dots \pm Px^3 \mp Qx^2 \pm Rx \mp S = 0$; assume the fraction

$$\frac{nx^{n-1} - n-1px^{n-2} + n-2qx^{n-3} - n-3rx^{n-4} + \&c.}{x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + \&c.} =$$

$$nx^{n-1} + (\alpha + \beta + \gamma + \delta + \&c.)x^{n-2} + (\alpha^2 + \beta^2 + \gamma^2 + \delta^2 + \epsilon^2 + \&c.)$$

$$x^{n-3} + (\alpha^3 + \beta^3 + \gamma^3 + \delta^3 + \epsilon^3 + \&c.)x^{n-4} + (\alpha^4 + \beta^4 + \gamma^4 + \delta^4 + \epsilon^4 + \&c.)$$

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&c.)

&c.) x^{-5} $(\alpha^{m-1} + \beta^{m-1} + \gamma^{m-1} + \&c. = P)x^{-m} + (\alpha^m + \beta^m + \gamma^m + \&c. = Q)x^{-m-1} + (\alpha^{m+1} + \beta^{m+1} + \gamma^{m+1} + \&c. = R)x^{-m-2} + \&c.$; then will $\frac{R}{Q}$ or $\sqrt[m]{Q}$ be the greatest root nearly.

Ex. 2. $\frac{R - 2Qx + 3Px^2 - \&c. \dots nx^{n-1}}{S - Rx + Qx^2 - Px^3 + \&c. \dots x^n} = \left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma} + \&c.\right) + \left(\frac{1}{\alpha^2} + \frac{1}{\beta^2} + \frac{1}{\gamma^2} + \frac{1}{\delta^2} + \&c.\right)x + \left(\frac{1}{\alpha^3} + \frac{1}{\beta^3} + \frac{1}{\gamma^3} + \&c.\right)x^2 + \left(\frac{1}{\alpha^4} + \frac{1}{\beta^4} + \frac{1}{\gamma^4} + \&c.\right)x^3 + \dots + \left(\frac{1}{\alpha^{m-1}} + \frac{1}{\beta^{m-1}} + \frac{1}{\gamma^{m-1}} + \&c. = P\right)x^{m-2} + \left(\frac{1}{\alpha^m} + \frac{1}{\beta^m} + \frac{1}{\gamma^m} + \&c. = Q\right)x^{m-1} + \left(\frac{1}{\alpha^{m+1}} + \frac{1}{\beta^{m+1}} + \frac{1}{\gamma^{m+1}} + \&c.\right)x^m + \&c.$; then will $\frac{P}{Q}$ or $\sqrt[m]{\frac{P}{Q}}$, the least root nearly.

Ex. 3. $\frac{1}{x^n - px^{n-1} + qx^{n-2} - \&c.} = x^{-n} + (\alpha + \beta + \gamma + \delta + \&c.)x^{-n-1} + (\alpha^2 + \beta^2 + \gamma^2 + \&c. (+\alpha\beta + \alpha\gamma + \beta\gamma + \alpha\delta + \&c.))x^{-n-2} + (\alpha^3 + \beta^3 + \gamma^3 + \&c. (+\alpha^2\beta + \alpha^2\gamma + \beta^2\gamma + \gamma^2\alpha + \gamma^2\beta + \&c.)) + \alpha\beta\gamma + \alpha\beta\delta + \alpha\gamma\delta + \beta\gamma\delta + \&c.))x^{-n-3} + \&c.$; and $\frac{S}{S - Rx + Qx^2 - \&c. \dots x^n} = 1 + \left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma} + \&c.\right)x + \left(\frac{1}{\alpha^2} + \frac{1}{\beta^2} + \frac{1}{\gamma^2} + \&c. (+\frac{1}{\alpha\beta} + \frac{1}{\alpha\gamma} + \frac{1}{\beta\gamma} + \&c.))x^2 + \&c.$ in each of which all the numeral co-efficients are 1. The approximate values to the greatest and least root may be found in the same manner as before.

From the preceding examples it appears, that the same observations which have been applied to Sir ISAAC NEWTON's method are equally applicable to M. BERNOULLI's.

13. In the *Meditationes* this rule is further extended, viz. let the given equation involve irrational and other functions of the unknown quantity; reduce it so that no function of the unknown quantity (x) may be contained in the denominator, and let the resulting equation be $A=0$. Assume a fraction

$$\frac{B}{A},$$

$\frac{B}{A}$, whose numerator B is a finite rational and integral function of the unknown quantity; reduce $\frac{B}{A}$ into a series proceeding according to the dimensions of the unknown quantity; for example, let the series be $A'x + B'x^{+1} + Cx^{+2} + \dots + Lx^{+4} + Mx^{+4+1} + Nx^{+4+2} + \&c.$; then (*exceptis excipiendis*) if s be negative, will the greatest root be $\sqrt[4]{\frac{M}{L}}$ nearly; but, if s be affirmative, $\sqrt[4]{\frac{L}{M}}$ will be the least root nearly. If l be infinite, then (*exceptis excipiendis*, as before-mentioned) the quantities $\sqrt[4]{\frac{M}{L}}$ and $\sqrt[4]{\frac{L}{M}}$ will be the above-mentioned roots accurately.

These principles have been applied to find the remaining roots of the given equation as well as the greatest and least.

14. The rule of false has been found very useful in finding approximates to the two unknown quantities contained in two given equations, and has been applied to (n) equations having (n) different unknown quantities: for example, it has been observed, that if two or more (m) values of an unknown quantity (x) are nearly equal to each other and to its given approximate value (x') , the unknown quantity $v = x - x'$ will ascend to two or more (m) dimensions in one of the resulting equations; or in more than one equations will be contained such powers of the quantity (v) , that if the more equations were reduced to one whose unknown quantity is v , the resulting equation will contain (m) dimensions of the quantity v . Hence it appears, that in this case also the convergency of the approximate values found will depend on the given approximate being much more near to one root than to any other.

15. When

15. When the given equations $A=0$, $B=0$, $C=0$, &c. contain irrational or other quantities whose fluxions can be found; and approximates (a , b , c , d , &c.) are given to each of the unknown quantities (x , y , z , v , &c.) contained in the given equations; let $a+x'=x$, $b+y'=y$, $c+z'=z$, $d+v'=v$, &c. ∴ substitute in the quantities $A, (\frac{A'}{x}), (\frac{A'}{y}), (\frac{A'}{z}), (\frac{A'}{v})$, &c.; $(\frac{\ddot{A}}{x^2}), (\frac{\ddot{A}}{xj}), (\frac{\ddot{A}}{xz}), (\frac{\ddot{A}}{xv}),$ &c.; $(\frac{\ddot{A}}{xy^2}), (\frac{\ddot{A}}{x^2j}),$ &c. for x, y, z, v , &c. respectively a, b, c, d , &c.; let the resulting correspondent values be $A', (\frac{A'}{x}), (\frac{A'}{y}),$ &c. whence may be deduced the equation $A' + (\frac{A'}{x}) x' + (\frac{A'}{y}) y' + (\frac{A'}{z}) z' + (\frac{A'}{v}) v' + \&c.) + (\frac{1}{2} (\frac{A''}{x^2}) x'^2 + \frac{1}{2} (\frac{A''}{j^2}) y'^2 + \frac{1}{2} (\frac{A''}{z^2}) z'^2 + \&c. + (\frac{A'}{xj}) x'y' + (\frac{A''}{xz}) x'z' + \&c.) + \&c. = 0$ in which no irrational, &c. function of the approximates $x', y', z',$ &c. is contained; and in the same manner may the remaining equations $B=0$, $C=0$, &c. be transformed into others, in which no irrational function of the approximates ($x', y', z',$ &c.) is contained, and from the resulting equations may be found approximate values of the quantities $x', y', z',$ &c.

If there be given only two equations $A=0$ and $B=0$ containing two unknown quantities x and y , and all the quantities of the resulting equations, in which are contained more than one dimension of the quantities x' and y' be rejected, there will result the two equations $A' + (\frac{A'}{x}) x' + (\frac{A'}{y}) y' = 0$ and $B' + (\frac{B'}{x}) x' + (\frac{B'}{y}) y' = 0$, from which may be found x' and y' , the same

same as given by Mr. SIMPSON and others. When two or more values of the quantity x are nearly $=a$, then in a resulting equation or equations, quantities of two or more dimensions of the approximate x' are to be included.

P A R T III.

1. THE first algebraists divided quantities, and extracted their roots no further than the quantities themselves: they did not perceive the utility of proceeding any further, otherwise the operation would have been the same continued. Mr. GREGORY ST. VINCENT, and Mr. MERCATOR divided, and Sir ISAAC NEWTON divided and extracted the roots of quantities (in which only one unknown quantity (x) is contained) by the operations then used by arithmeticians, into series ascending or descending, according to the dimensions of x in *infinitum*. They clearly saw the utility of it in finding the fluents of fluxions, as Dr. WALLIS and others some little time before had found the fluent of the fluxion $ax^{\frac{m}{n}}x$; or, which is the same, the area of a curve whose ordinate is $ax^{\frac{m}{n}}$ and abscissa is x .

2. M. LEIBNITZ asked from Mr. NEWTON the cases in which the above-mentioned serieses would converge; for it would be altogether useless when it diverges, and of little use when it converges slowly.

Cas. 1. To this question an answer, I believe, was first given in the *Meditationes*, viz. reduce the function to its lowest terms; and also in such a manner that the quantities contained in the numerator and denominator may have no denominator: make the denominator $Q=0$, and every distinct irrational quantity contained in it $=0$; and also every distinct irrational quantity H contained in the numerator $=0$; then, let α be the least root affirmative or negative (but not $=0$) of the above-mentioned resulting equations, the ascending series will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the above-mentioned series will not converge.

If the above-mentioned series (S) be multiplied into \dot{x} , and its fluent found; then will the series denoting the fluent contained between two values a and b of the quantity (x) converge, when a and b are both contained between α and $-\alpha$: the fluent always converges faster than the series S, the unknown quantity x having the same values in both.

Ex. Let $\int \frac{\dot{x}}{a+bx+cx^2+\dots x^n} = Ax + \frac{1}{2}Bx^2 + \frac{1}{3}Cx^3 + \&c.$ and the roots of the equation $a+bx+cx^2+\dots x^n=0$ be $\alpha, \beta, \gamma, \delta, \&c.$; then $\frac{\dot{x}}{a+bx+cx^2+\dots x^n} = \frac{\pi\dot{x}}{\alpha-x} + \frac{\rho\dot{x}}{\beta-x} + \frac{\sigma\dot{x}}{\gamma-x} + \&c. = A\dot{x} + Bx\dot{x} + Cx^2\dot{x} + \&c.$; but $\frac{\pi}{\alpha-x} = \frac{\pi}{\alpha} + \frac{\pi x}{\alpha^2} + \frac{\pi x^2}{\alpha^3} + \&c.$ in infinitum, $\frac{\rho}{\beta-x} = \frac{\rho}{\beta} + \frac{\rho x}{\beta^2} + \frac{\rho x^2}{\beta^3} + \&c.$ &c.; the former series converges when x is contained between α and $-\alpha$, the latter when x is between β and $-\beta$, and so on. In the same manner the fluents $\int \frac{\pi\dot{x}}{\alpha-x} = \frac{\pi}{\alpha}x + \frac{\pi x^2}{2\alpha^2} + \&c.$ $\int \frac{\rho\dot{x}}{\beta-x} = \frac{\rho}{\beta}x + \frac{\rho x^2}{2\beta^2} + \&c.$ &c. *a fortiori* converge when x is between α and $-\alpha$, β and $-\beta$, &c. respectively, and so on: hence the series $Ax + \frac{1}{2}Bx^2 + \frac{1}{3}Cx^3 + \&c.$

$$= \int \frac{x}{a+bx+cx^2 \dots \alpha^n} = \int \frac{\pi x}{a-x} + \int \frac{ex}{\beta-x} + \int \frac{\sigma x}{\gamma-x} + \&c. = \left(\frac{\pi}{a} + \frac{e}{\beta} + \frac{\sigma}{\gamma} + \&c. \right) x + \left(\frac{\pi}{a^2} + \frac{e}{\beta^2} + \frac{\sigma}{\gamma^2} + \&c. \right) x^2 + \&c.$$
 always converges when x is between α and $-\alpha$, where α is the least root of the above-mentioned equation; but where x is greater than α or $-\alpha$, the series will diverge.

The infinite series $a^m + ma^{m-1}x + m \cdot \frac{m-1}{2} a^{m-2}x^2 + \&c. = \overline{a+x}^m$ will always converge when a is greater than x , and diverge when less, and consequently its convergency does not depend on the index m , unless when $x = \pm a$: and in the *Meditationes Analyticae* are given the cases in which it converges or diverges when $\mp a = x$; and also if the series $x^m + max^{m-1} + \&c. = \overline{x+a}^m$ descends according to the dimensions of x , when it converges or diverges.

Caf. 2. Let the above-mentioned quantity be reduced into a series $Ax^r + Bx^{r-1} + \&c.$ descending according to the dimensions of the unknown quantity x ; then will the series $Ax^r + Bx^{r-1} + \&c. = P$, or the series $\frac{Ax^{r+1}}{1-r} - \frac{Bx^r}{r} + \&c. = \int Px$ converge, when x is greater than the greatest root (λ) of the above-mentioned equations, and diverge when it is less; and consequently in this case, when the fluent is required between the two values a and b of x ; the series found will converge when a and b are both greater than λ .

Caf. 3. When x is equal to the least root in the former case, and to the greatest in the latter, then sometimes the series will converge, and sometimes not. These different cases are given in the *Meditationes*; but it would be too long to insert them in this Paper.

4. If any roots are impossible as $a - b\sqrt{-1}$ and $a + b\sqrt{-1}$, then the series will converge when x is in the first case less

less than $\pm(a-b)$, and in the second case greater than $\pm(a+b)$; or, more general, it will converge in the first case when x^n is always infinitely less than the reciprocal of the quantity $\frac{(a+b\sqrt{-1})^n + (a-b\sqrt{-1})^n}{(a^2+b^2)^n} = P$, where n is infinite; and in the latter case it will converge when x^n is infinitely greater than P .

It may not be improper to observe, that the same values of the root are to be applied in the equations, which are applied in the series.

3. Sir ISAAC NEWTON, in the binomial theorem, reduced the power or root of a binomial into a series proceeding according to the dimensions of the terms contained in the binomial. M. DE MOIVRE reduced the power or root of a multinomial into a like series; but in all cases, except the most simple, we must still recur to the common division, extraction of roots, &c.

4. Mess. EULER, MACLAURIN, and other mathematicians, finding that the serieses before-mentioned often converged slowly, or, if the truth may be confessed, commonly not at all, to deduce the area of a curve contained between two values a and b , of the abscissæ, or fluent of a fluxion between two values a and b of the variable quantity x , interpolated the series or area, between a and b ; that is, found the area or fluent contained between the abscissæ a and $a+\alpha$, then between the abscissæ $a+\alpha$ and $a+2\alpha$, and then between the abscissæ $a+2\alpha$ and $a+3\alpha$, and so on, till they came to the area between $b-\alpha$ and b . M. EULER observed, that when the ordinate became 0 or infinite, the series expressing the area converges slowly; and therefore,

In order to investigate the area near the points of the abscissæ, where the ordinates become 0 or infinite, he transforms the equation, and finds serieses expressing the area near those points; in which serieses the abscissæ or unknown quantities begin from those points.

5. In the *Meditationes* it is asserted, that in a series proceeding according to the dimensions of x , if any root of the above-mentioned equations be situated between the beginning of the abscissæ 0 and its end x , the series will not converge: it is therefore necessary to transform the abscissæ so that it may begin or end at each of the roots of the above-mentioned equations, and consequently where the ordinates become 0 or infinite, &c.; those cases excepted where the ordinate becomes 0, and its correspondent abscissa is a root of a rational function W of x without a denominator, and $\int WPx$ is equal to the given series; and in general the abscissæ ought to begin from the above-mentioned points; for if they end there the series will converge very slow, if at all.

6. It is further asserted, that if a and b , the values of the abscissæ between which the area is required, be both more near to one root of the above-mentioned equations than to any other, and n serieses are to be found, whose sum expresses the area contained between a and b ; then that the sum of the (n) serieses may converge the swiftest, the distances of the beginnings of each of the n abscissæ from the adjacent root will form a geometrical progression.

7. Mr. CRAIG found the fluent of any fluxion of the formula $(a + bx^m + cx^{2m} + \&c.)^m x^{m-1}$ by a series of the following

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kind

kind $(a + bx^n + cx^{2n} + \&c.)^{n+1} \times x^0 \times (a + \beta x^n + \gamma x^{2n} + \&c. \text{ in infinitum})$. Sir ISAAC NEWTON, by serieses of the same kind, found the fluents of fluxions of this formula $(a + bx^n + cx^{2n} + \&c.)^m \times (e + fx^n + gx^{2n} + \&c.)^n \times \&c. x^{q-1} \dot{x}$; the same principle is extended somewhat more general in the *Meditationes*.

8. Mr. JOHN BERNOULLI found the fluent of any fluxion $\int nx = nx - \frac{x^{2n}}{2n} + \frac{x^{3n}}{2 \cdot 3n^2} - \&c.$ from the principles which Mr. CRAIG published for finding the fluents of fluxions involving fluents.

In the *Meditationes* something is added of the convergency of these series; and also,

9. In them a new method is given of finding approximations. Let some terms in the given quantity be much less or greater than the rest; then reduce the quantity into terms proceeding according to the dimensions of the small quantities, or according to the reciprocals of the great quantities, and it is done. If the fluent of the quantity resulting is required, find it from the common methods, if possible; but if not, reduce the terms not to be found into an infinite series, and then find approximate values to each of the terms, &c.

Ex. 1. Let R the radius, and A the arc of a circle whose sine is S and cosine C, and $A \pm e$ an arc of a circle which does not much differ from the arc A, that is, let e be a very small quantity; then will the sine of the arc $A \pm e$ be $S(1 - \frac{1}{2} \times \frac{e^2}{R^2} + \frac{1}{2 \cdot 3 \cdot 4} \times \frac{e^4}{R^4} - \frac{1}{2 \cdot 3 \cdot 4 \cdot 5 \cdot 6} \times \frac{e^6}{R^6} + \&c.) \pm C(\frac{e}{R} - \frac{1}{2 \cdot 3} \times \frac{e^3}{R^3} + \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} \frac{e^5}{R^5} - \&c.)$; and the cosine of the same arc will be $C(1 - \frac{1}{2} \times \frac{e^2}{R^2} + \frac{1}{2 \cdot 3 \cdot 4} \times \frac{e^4}{R^4} - \&c. \text{ in infinitum}) \pm S(\frac{e}{R} -$

$$\frac{1}{2 \cdot 3} \times \frac{e^3}{R^3} + \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} \times \frac{e^5}{R^5} - \&c.).$$

Ex. 2. Log. $(x \pm e) = \int \frac{\dot{x}}{x \pm e} = \log. x \pm \frac{e}{x} \mp \frac{e^2}{2x^2} \pm \frac{e^3}{3x^3} \mp \&c.:$

$$\int \frac{\dot{x}}{a^2 - (x+e)^2} = \int \frac{\dot{x}}{a^2 - x^2} + \frac{e}{a^2 - x^2} + \&c.$$

Ex. 3. $\int \frac{\dot{x}}{a^2 + (x-e)^2} = \int \frac{\dot{x}}{a^2 + x^2} - \frac{e}{a^2 + x^2} + \&c.$

Ex. 4. $\int \frac{j}{\sqrt{(1-(y \pm e)^2)}} = \int \frac{j}{\sqrt{1-y^2}} \pm \frac{e}{\sqrt{1-y^2}} \pm \&c.$

Ex. 5. $\int \frac{j}{\sqrt{1-y^2} + e} = \int \frac{j}{\sqrt{1-y^2}} - \int \frac{e j}{1-y^2} + \int \frac{e^2 j}{(1-y^2)^{3/2}} - \int \frac{e^3 j}{(1-y^2)^{5/2}} + \&c.$

Ex. 6. Let the fluxion be $\frac{\sqrt{1-cx^2}\dot{x}}{\sqrt{1-x^2}}$, where c is a very small

quantity; then, if P be put for $\sqrt{1-x^2}$, the fluxion becomes

$$\frac{\dot{x}}{P} - \frac{cx^2\dot{x}}{2P} - \frac{c^2x^4\dot{x}}{8P} - \frac{c^3x^6\dot{x}}{16P} - \&c. \text{ of which the fluents will be found}$$

$$A - \frac{c}{2} \times \frac{1 \cdot A - xP}{2} - \frac{c^2}{8} \times \frac{3B - x^3P}{4} - \frac{c^3}{16} \times \frac{5C - x^5P}{6} - \&c. \text{ where } A = \int \frac{\dot{x}}{P}, B = \frac{1 \cdot A - xP}{2}, C = \frac{3B - x^3P}{4}, \&c.$$

This is the swiftest converging series for finding the length of the arc of an ellipse nearly circular, which is yet known; for example, let the absciss to the axis beginning from the center = x , the semi-transverse axis of the ellipse be 1, its semi-conjugate $1-d$; then will $c = 2d - d^2$, and let the length of the quadrant of the ellipse be required, in this case $x = 1$, and $P = \sqrt{1-x^2} = 0$; and $A = \frac{3,14159, \&c.}{2} = 1,57079,$

$\&c.$ whence the length required is $1,57079, \&c. \times (1 - \frac{c}{2 \cdot 2} -$

$$\frac{1 \cdot 3c^2}{2^2 \cdot 4^2} - \frac{1 \cdot 3^2 \cdot 5c^2}{2^2 \cdot 4^2 \cdot 6^2} + \frac{1 \cdot 3^2 \cdot 5^2 \cdot 7c^4}{2^4 \cdot 4^2 \cdot 6^2 \cdot 8^2} - \&c.).$$

EX. 7. Let the fluxion be $\dot{x}(a^3 + a' + (3a^2b + b')x + (3ab^2 + c')x^2 + (b^3 + d')x^3)^m = P\dot{x}$, where a' , b' , c' , &c. are very small quantities; then will $P\dot{x} = ((a + bx)^{3m} + ma + bx^{3m-3} \times (a' + b'x + c'x^2 + d'x^3) + m \cdot \frac{m-1}{2} \overline{a + bx}^{3m-6} \times (a' + b'x + c'x^2 + d'x^3)^2 + \&c.) \dot{x}$,

of which the fluent is $\frac{1}{3m+1 \cdot b} \overline{a + bx}^{3m+1} + m(a + bx)^{3m-1} \times \left(\frac{a'}{(3m+1)b} x^3 + \frac{c' - 3aA}{3mb} x^2 + \frac{b' - 2aB}{(3m-1)b} x + \frac{a' - aC}{(3m-2)b} \right) + \&c.$ where the letters A, B, C, &c. denote the preceding co-efficients.

10. M. EULER and others, reduced the series $Ax^r + Bx^{r+1} + Cx^{r+2} + \&c.$ into a series $A' \sin. r\alpha + B \sin. r + s\alpha + \&c.$ &c. where α denotes the arc of a circle, whose sine is ax , &c. It may be easily reduced into infinite other serieses proceeding according to the dimensions of quantities, which are functions of x ; but it is most commonly preferable to reduce it into serieses proceeding according to the sines, cosines, tangents, or secants of the arcs of circle, which sines, &c. can immediately be procured from the common tables.

It has been observed in the first part, that to find the root of an equation, an approximate value much more near to one root of the equation than to any other must be given. In this part it is further observed, that serieses deduced from expanding given quantities, so as to proceed according to the dimensions of the unknown or variable quantities, will not converge if the unknown quantities be greater than the least roots of the above-mentioned equations; and that they will not converge much, unless the unknown quantities have a small proportion to the least roots; and if the given quantities be expanded into serieses descending according to the dimensions of the unknown quantities, then the serieses resulting will not converge if the greatest

greatest roots of the equations before-mentioned be greater than the unknown quantities; and unless the unknown quantities have a great ratio to the greatest roots the serieses will converge slowly: for example, the serieses $\int \frac{x}{1+x} = x - \frac{1}{2}x^2 + \&c.$, $\int \frac{z}{1+z^2} = z - \frac{1}{2}z^3 + \frac{1}{4}z^5 - \&c.$, $\int \frac{y}{\sqrt{1-y^2}} = y + \frac{1}{2}y^3 + \&c.$ will never converge if x , z , or y , be greater than 1; but will always converge when less than ± 1 or $\pm 1 \sqrt{-1}$ the least or only roots of the equations $1+x=0$, $1-y^2=0$, and $1+z^2=0$. The series $y + \frac{1}{2}y^3 + \&c.$ will always converge when y is situated between $+1$ and -1 , in which case alone it is possible. The same is true also of a series arising from expanding the $\int (ax^m + bx^{m-1} + cx^{m-2} + \&c.) \frac{1}{x}$ into a series proceeding according to the dimensions of x , if the equation $ax^m + bx^{m-1} + cx^{m-2} + \&c. = 0$ has only two possible roots α and $-\alpha$, which are less in the manner before-mentioned than any impossible root contained in it.

If in either of the above-mentioned serieses the unknown quantity x , z , or y , has a great proportion to 1, the series will converge very slow; for example, if $x=1$, ten thousand numbers at least are to be calculated, to procure the sum of the series true to four figures; therefore, in these and most other serieses it is necessary first to find a near value, viz. when x either $=z$, when e is very small; or $=e$, when z is very small; and then write $z+e$ for x in the quantity, and reduce it in the former case into a series proceeding according to the dimensions of e , in the latter case according to the dimensions of z , and there will arise two serieses, of which the fluents properly corrected, viz. by adding the fluent contained between the values a and e to the latter, and that between a and z to the former, will give the same fluent.

fluent. The first term of the series, in which e is supposed very small, will be the fluent of the given fluxion, when $x = z$.

11. If a fluxion $P\dot{x}$, where P is a function of x , be transformed into another $Q\dot{z}$, where Q is a function of z , and they be reduced into serieses A and B , proceeding according to the dimensions of x and z respectively; find α and π , correspondent values of the quantities x and z ; then in ascending serieses, if α has a less ratio to the least root of the equation $P=0$, than π has to the least root of the equation $Q=0$, the series A (*exceptis excipiendis*) will converge swifter than the series B .

12. Dr. BARROW, in some equations, expressing the relation between the absciss x and ordinate y , found y in the two first terms of x , viz. $y = a + bx$, which is an equation to the asymptotes of the curves. Sir ISAAC NEWTON, from an algebraical equation given, expressing the relation between y and x , found a series proceeding according to the dimensions of x , expressing y in terms of x . M. LEIBNITZ performed the same problem by assuming a series $Ax^n + Bx^{n+r} + Cx^{n+2r} + \&c.$ with general co-efficients, and substituting this series for y in the given equation, &c. from equating the correspondent terms he deduced the indexes and co-efficients. M. DE MOIVRE, Mr. MAC LAURIN, &c. observed, that when the highest terms of the given equations have two or more (m) divisors equal; for example, $(y - ax^n)^m$; to which we must add, and when a value of y in this case is required nearly equal to Ax^n , a series $Ax^n + Bx^{n+\frac{r}{m}} + \&c.$ is to be assumed, whose indexes differ only by $\frac{r}{m}$, &c. if otherwise they would differ by r .

This reduction seldom answers any other purpose than finding the fluents of fluxions as $\int y\dot{x}$, &c.; or the asymptotes, &c.

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of curves, which depend on some of the first terms of the series; but will very seldom be used for finding the roots of an equation; the rule of false, or method given by VIETA, will ever be substituted in its stead.

13. The values of x may be required between which the above-mentioned series $Ax^n + Bx^{n+r} + Cx^{n+2r} + \&c.$ will converge, as the infinite series answers no purpose when it diverges. First, if an ascending be required, write for y in the given algebraical equation an infinite quantity, and find the roots of x in the equation thence resulting $P=0$; which for y write in the same equation, and find the roots of x in the resulting equation which contain irrational quantities, *viz.* if one root be $x=a$; then let it contain $(x-a)^m$, where m is not a whole number; find the roots of the equations resulting from making every irrational function of (x) contained in the given equation $=0$, there being no irrational function of y contained in it; then, if x be greater than the least root not $=0$ of the above-mentioned equations, the series will not converge; but if it be a series descending according to the dimensions of x , and x be less than the greatest root of the above-mentioned equations, the series will not converge.

In interpolating serieses to investigate the fluent contained between two values a and b of the fluxion $(Ax^n + Bx^{n+r} + \&c.)dx$, it is preferable to make the abscissæ begin from every one of the above-mentioned roots contained between a and b .

Most commonly these serieses will not converge unless x be less, &c. than other quantities not investigated by this rule.

14. Sir ISAAC NEWTON gave an elegant example of this rule in the reversion of the series, $y = ax + bx^2 + cx^3 + \&c.$ from which the investigation of the law of the series has never been attempted. In the year 1757 I sent the first edition of my

Meditationes Algebraicæ to the Royal Society, and published it in 1760, and afterwards in 1762, with another part added, on the Properties of curves, under the title of *Miscellanea Analytica*, in all which was given the law of a series for finding the sum of the powers of the roots of an equation from its co-efficients. That great mathematician M. LE GRANGE and myself printed about the same time an observation known to me at the time that I printed the above-mentioned book, that the law of its powers and roots, if it proceeds in *infinitum*, is the same; from which series of mine, with great sagacity, M. LE GRANGE found the law which Sir ISAAC NEWTON's reversion of series observes. In the *Meditationes* the law is given, and the series is made to proceed according to the dimensions of x , &c.

15. It is asserted in the *Meditationes*, that in most equations of high dimensions, unless purposely constituted, the sum of the terms which, from the given hypothesis, become the greatest, being supposed $= 0$, only an approximate to the value Ax^n of y in the resulting equation can by the common algebra be deduced. In this case the approximate to the quantity A is to be found so near as the approximate value of the quantity sought requires; or perhaps it is preferable to correct in every operation the approximate values of the quantities A, B, C , &c. in the series required $A'x^n + B'x^{n+r} + C'x^{n+2r} + \&c.$

In the equation the quantity $z \pm e$ may be substituted for x , and from the equation resulting a series expressing the value of y may be found, proceeding either according to the dimensions of the quantity z , or its reciprocals, according to the conditions of the problem.

16. If there are more than one (n) equations having $(n+1)$ unknown quantities (x, y, z , &c.): in each of the equations for y ,

$z,$

x , &c. write respectively Ax^n , $A'x^m$, &c.; and suppose the terms of each of the equations, which result the greatest from the given or assumed hypothesis $= 0$, and from the resulting equations may be found the first approximates Ax^n , $A'x^m$, &c. either accurately or nearly; then, in the given equations for y , z , &c. write $y' + (A + a)x^n + Bx^{n+n'} + \&c. z' + (A' + a')x^m + B'x^{m+m'}$, where a , a' , &c. are very small quantities; and suppose the terms of each of the equations which become greatest from the above-mentioned hypothesis respectively $= 0$, and from the equations resulting deduce the quantities a , a' , &c.; n' , m' , &c.; B , B' , &c.; and so on: or assume $y = (A + 1a + a1 + \&c.)x^n + (B + 1b + b1 + \&c.)x^{n+n'} + \&c.$; $z = (A' + 1a' + a'1 + \&c.)x^m + (B' + 1b' + b'1 + \&c.)x^{m+m'} + \&c.$ &c.; substitute these quantities for their values in the given equations, and from equating the correspondent terms of the resulting equations may be deduced the quantities required.

The differences of the indexes n' , &c. m' , &c. may be deduced by writing x^n , x^m , &c. for y , z , &c. in the given equations, from the differences of the indexes of the quantities resulting. The same principles may be applied in finding the above-mentioned differences, when two or more values are Ax^n , &c. which were applied in a like case to one equation having two unknown quantities.

The same principles which discover the cases in which a series deduced from an equation having two unknown quantities will converge, may be applied for the same purpose to these series.

17. In the equations for x , y , z , &c. write respectively $x' + e$, $y' + f$, $z' + g$, &c.; and from the equations resulting find y' , z' , &c. in serieses either proceeding according to the dimensions

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of the quantities $e, f, g, \&c.$; or according to the dimensions of the quantity x' , as the conditions of the problem require.

18. Given one or more (n) algebraical equations involving ($n+m$) unknown quantities, one unknown quantity (y) may be expressed by a series proceeding according to the dimensions of the $m-1$ remaining ones ($x, z, v, \&c.$), in which any dimensions of $z, v, \&c$ assumed may be supposed to correspond to (l) dimensions of the quantity (x).

19. In a fluxional equation of the m^{th} order, expressing the relation between x, y , and their fluxions, where \dot{x} is constant, Mr. EULER substitutes in the given equation for $\dot{y}^m, \dot{y}^{m-1}, \dot{y}^{m-2}, \dot{y}^{m-3}, \&c.$ respectively $Ax^{n-m}\dot{x}^m, \frac{A}{n-m+1}x^{n-m+1}\dot{x}^{m-1} + a\dot{x}^{m-1},$

$$\frac{A}{(n-m+1)(n-m+2)}x^{n-m+2}\dot{x}^{m-2} + ax\dot{x}^{m-2} + b\dot{x}^{m-2}, \frac{A}{(n-m+1)(n-m+2)}$$

$$\frac{A}{(n-m+3)}x^{n-m+3}\dot{x}^{m-3} + \frac{1}{2}ax^2\dot{x}^{m-3} + bx\dot{x}^{m-3} + c\dot{x}^{m-3}, \&c. \text{ where}$$

$a, b, c, \&c.$ are any quantities to be assumed in such a manner as the conditions of the problem require; from supposing the aggregate of the terms of the resulting equation, which are the greatest, $=0$, may be deduced the first approximate Ax^n , or else (as is beforementioned) $A'x^n$ a near approximate to Ax^n , and by proceeding as in algebraical equations another approximate may be found, and so on. The same may be found by assuming $y = Ax^n + Bx^{n+r} + Cx^{n+2r} + \&c. + ax^m + bx^{m-1} + cx^{m-2} + \&c.$ or $y = (A + 1a + a_1 + \&c.)x^n + (B + 1b + b_1 + \&c.)x^{n+r} + (C + 1c + c_1 + \&c.)x^{n+2r} + \&c. + ax^m + bx^{m-1} + cx^{m-2} + \&c.$ and substituting it and its fluxions for their values $y, \dot{y}, \ddot{y}, \&c.$ in the given equation, and supposing the aggregate of each correspondent terms, which do not very much differ from each other, $=0$; from the resulting equations can be deduced the co-efficients $A, B, C, \&c.$; or $A, 1a, a_1, \&c.$; $B, 1b, b_1, \&c.$; $C, 1c, c_1, \&c. \&c.$

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In the given equation for y , x , and their fluxions substitute $y' + f$, $x' + g$, and their fluxions, where the quantities f and g , &c. are assumed, so as to render the quantities y' and x' , &c. very small.

In finding the series which expresses the value of y in terms of x , there will always occur as many invariable quantities to be assumed at will as is the order of the fluxional equation, provided the series begins from its first terms; and to find them there will result equations easily reducible to homogeneous fluxional equations, of which the orders do not exceed m .

V. *Experiments on Hepatic Air.*By Richard Kirwan, *Esq. F. R. S.*

Read December 22, 1785.

HEPATIC Air is that species of permanently elastic fluid which is obtained from combinations of sulphur with various substances, as alkalies, earths, metals, &c. It possesses many peculiar and distinct properties; among which the most obvious are, a disagreeable characteristic smell emitted by no other known substance; inflammability, when mixed with a certain proportion of respirable or nitrous air; miscibility with water, to a certain degree; and a power of discolouring metals, particularly silver and mercury. These properties were first discovered by that incomparable analyst M. SCHEELÉ.

This air acts an important part in the œconomy of nature. It is frequently found in coal-pits; and the truly excellent and ever to be regretted M. BERGMAN has shewn it to be the principle on which the sulphureous properties of many mineral waters depend, and thus happily terminated the numerous disputes which the obscurity of that subject had occasioned. There is also great reason to think, that it is the peculiar product of the putrefaction of many, if not all, animal substances. Rotten eggs and corrupt water are known to emit the smell peculiar to this species of air, and also to discolour metallic substances

stances in the same manner. M. VIELLARD has lately discovered several other indications of this air in putrefied blood.

Yet, deserving as this substance appears to be of a thorough investigation, it has as yet been very little attended to. The experiments of M. BERGMAN have not been sufficiently numerous, and thereby led him into some mistakes. Dr. PRIESTLEY has intirely overlooked it. The researches of the ingenious M. SENNEBIER, of Geneva, have indeed been very extensive; but as, for particular reasons, he operated on this air over water (by which it is in great measure absorbed) instead of quicksilver, his conclusions appear in many respects objectionable, as will be seen in the sequel. The experiments I have now the honour of laying before the Society were all made over quicksilver, and several times repeated.

SECTION I.

Of the Substances that yield Hepatic Air, and the means of obtaining it.

It is well known, that *saline* liver of sulphur is formed, in the dry way, of a mixture of equal parts of vegetable or mineral alkali and flowers of sulphur, melted together by a moderate heat, in a covered crucible. I examined the circumstances of its formation, and observed, that when this mixture was slightly heated, it emitted a bluish smoke, which gradually grew whiter as the heat was increased, and at last, when the mixture was perfectly melted, and the bottom of the crucible slightly red, became perfectly white and inflammable. To examine the nature of this smoke, I made a pretty pure fixed alkali, by deflagrating equal parts of cream of tartar and
nitre

nitre in a red-hot crucible in the usual way; and mixing this salt perfectly dry with flowers of sulphur in much smaller quantity, as I believe (for I did not weigh the salt, least it should, during the weighing, attract moisture) I gradually heated the mixture in a small coated glass retort, and received the air proceeding from it over quicksilver.

The first portion of air that passed with a very gentle heat was that of the retort itself, slightly phlogisticated. It amounted to 1,5 cubic inches, and with Dr. PRIESTLEY's nitrous test (that is, an equal measure of nitrous air) its goodness was 1,29. It contained no fixed air.

The second portion of air obtained by increasing the heat amounted to about 18 cubic inches. It was of a reddish colour, and seemed a mixture of nitrous and common air. It slightly acted on the mercury.

The third portion consisted of 20 cubic inches, and appeared to be of the same kind as the last, but mixed with a little fixed air.

This was succeeded by 64 cubic inches of almost perfectly pure fixed air; and the bottom of the retort being now red, some sulphur sublimed in its neck. When all was cold, liver of sulphur was found in the bulb of the retort.

Hence we see, that the blue smoke consists chiefly of fixed air, and the white or yellow smoke of sulphur sublimed; and that no hepatic air is thus formed; nor vitriolic air, unless the retort be so large as to contain a sufficiency of common air to admit the combustion of part of the sulphur.

2dly, That the aerial or any other acid, combined with the alkali, must be expelled before the alkali will combine with the sulphur. Liver of sulphur exercises a strong solving power on the earth of crucibles, and readily pierces through them.

The

The above experiment seems to shew that liver of sulphur will not yield hepatic air without the addition of an acid; and I believe this to be true when the experiment is made in the dry way, and nearly so in the moist way; for having added 200 grains of sulphur to a concentrated solution of strong caustic vegetable alkali, by a strong and long-continued heat I obtained only 1 cubic inch of hepatic air; yet it is well known, that a strong solution of liver of sulphur constantly emits an hepatic smell, even in the temperature of the atmosphere; and the substance so emitted contains so much hepatic air as to discolour silver and lead, and even their solutions; which shews, that an incomparably small quantity of this air is capable of producing this effect. To discover whether this extrication of hepatic air might be caused by the deposition of fixed air from the atmosphere, I threw some pulverised calcareous hepar into aerated water, and by the application of heat endeavoured to obtain hepatic air, but in vain: and, indeed, the very circumstance that the hepatic smell, and its effects, are always strongest the first instant that a bottle of the hepatic solution is opened, seems to indicate that fixed air is no way concerned in its production.

The best liver of sulphur is made of equal parts of salt of tartar and sulphur; but as about one-fifth of the salt of tartar consists of air which escapes during the operation, it seems, that the proportion of sulphur predominates in the resulting compound; yet as some of the sulphur also sublimes and burns, it is not easy to fix the exact proportion. 100 grains of the best, that is, the reddest liver of sulphur, afford, with dilute marine acid, about 40 cubic inches of hepatic air, in the temperature of 60°: a quantity equivalent to about 13 grains of sulphur, as will be seen in the sequel.

The *marine* acid is the best adapted to the production of hepatic air. If the concentrated *nitrous* acid be used, it will afford nitrous air; but having diluted some nitrous acid, whose specific gravity was 1,347, with 20 times its bulk of water, I obtained, with the assistance of heat, as pure hepatic air as with any other acid.

The concentrated *vitriolic* acid, poured on liver of sulphur, affords but little hepatic air without the assistance of heat, though it instantly decomposes the liver of sulphur; and it is partly for this reason that the proportion of air is so small; for it is during the *gradual* decomposition of sulphureous compounds that hepatic air is produced.

Distilled *vinegar* extricates this air in the temperature of the atmosphere; but it is not pure, its peculiar smell being mixed with that of vinegar.

The *acid of sugar* also produced some quantity of this air in the temperature of 59°.

20 grains of *sedative salt*, or acid (as it should more properly be called) dissolved in an ounce of water, being poured on liver of sulphur, afforded hepatic air only when in a boiling heat, or nearly so.

Neither the *aërial* nor *arsenical* acids produce any.

Liver of sulphur is soluble not only in water but in spirit of wine, and in caustic volatile alkalies; and the colour of both solutions is red. Sulphur is precipitable from the former by the addition of water or of an acid, but from the latter only by an acid.

Having made some liver of sulphur, in which the proportion of sulphur much exceeded that of the alkali, I poured on part of it some oil of vitriol, whose specific gravity was 1,863: by this means I obtained hepatic air, so loaded with

with sulphur, that it deposited some in the tube through which it was transmittted, and on the upper part of the glass receiver. This air I transferred to another receiver; but though it was then perfectly clear and transparent, and amounted to 6 cubic inches, yet the next morning the inside of the glass was thickly lined with sulphur, and the air reduced to 1 cubic inch, which was pure vitriolic air. Hence it appears, first, that a species of elastic fluid may exist in a state intermediate between the aërial and the vaporous, which is not permanently elastic like air, nor immediately condensed by cold like vapour, but which, by the gradual loss of its specific heat, may be reduced to a concrete form. 2dly, That so large a quantity of sulphur may be combined with vitriolic air, as to enable it to exhibit the properties of hepatic air, for some time at least. A mixture of three parts pulverised quick-lime and one part sulphur, heated to whiteness in a covered crucible for one hour, became of a stony hardness, and being treated with marine acid, afforded hepatic air. If a piece of this stone be heated in pure water it becomes bluish, and hence the origin of the blue marles generally found near hot sulphurated waters.

A calcareous hepar may also be formed in the moist way, as is well known.

Magnesia free from fixed air, heated in the same manner with sulphur, afforded no hepatic air when an acid was poured on it.

I also procured this air from a mixture of three parts *filings of iron* and one of sulphur, melted together, and treated with marine acid. It is remarkable, that this sulphurated iron, dissolved in marine acid, affords scarce any inflammable, but mostly hepatic air.

A mixture of equal parts filings of iron and sulphur, made into a paste with water, after heating and becoming black, afforded hepatic air when an acid was poured on it; but this hepatic air was mixed with inflammable air, which probably proceeded from the uncombined iron. After a few days, this paste lost its power of producing hepatic air.

M. BERGMAN has remarked, that combinations of sulphur with some other metals yield hepatic air.

I attempted extracting air from a mixture of oil of olives with caustic vegetable alkali. It immediately whitened, and on applying heat effervesced so violently as to pass over into the receiver: nor had I better success on adding an acid, as I might well foresee. The event was different when on a few grains of sulphur I poured some of the oil, and heated them in a phial with a bent tube; for as soon as the sulphur melted, the oil began to act on it, grew red, and emitted hepatic air, similar to that produced by other processes.

I also obtained this air in great plenty from a mixture of equal parts sulphur and *pulverised charcoal*, out of which its adventitious air had been as much as possible expelled by keeping it a long time heated to redness, in a crucible on which a cover was luted, with a small perforation to permit the air to escape. This air was inflammable, as appeared by holding a lighted candle before it during its emission; yet it is hardly possible to free charcoal wholly from foreign air, for it soon re-attracts it when exposed to the atmosphere.

This last mixture, when distilled, affording a large quantity of hepatic and some inflammable air, without the addition of any acid, I imagined, that as the retort was only half full, it might contain a sufficiency of atmospheric air to admit the combustion of part of the sulphur, and thus furnish the neces-

sary

fary acid; but when I filled the retort with air phlogisticated by the nitrous test unto 1,8, and in this air distilled the above mixture, the result was exactly the same as when the retort was filled with atmospheric air.

Six grains of *pyrophorus*, made of alum and sugar, effervesced with marine acid, and afforded 2,5 cubic inches of hepatic air. This pyrophorus had been made six years before, and was kept in a tube hermetically sealed, and for many summers exposed to the strongest light of the sun. It was so combustible, that some grains of it took fire while it was introduced into the phial out of which the hepatic air was expelled.

A mixture of two parts *white sugar* (previously melted in order to free it from water) with one part sulphur, when heated to about 600 or 700 degrees, gave out hepatic air very rapidly. This air had a smell much resembling that of onions; it contained no fixed air, nor saccharine, or other acid. But sugar and sulphur, melted together, gave out no hepatic air when treated with acids. Water, spirit of wine, and marine acid, decompose this mixture, dissolving the sugar, and leaving the sulphur.

A mixture of sulphur and *plumbago* afforded me no hepatic air.

I then tried whether sulphur could combine with elastic fluids, and the results were as follows.

12 grains of sulphur, heated in a retort, filled with metallic *inflammable* air, afforded no hepatic air; though when the retort was cold, and for some time exposed to the air, it smelled of hepatic air. It is true, the heat I applied might be insufficient; for the inflammable air passing over with a slight heat, the mercury ascended so high into the neck of the receiver, that, fearing the rupture of the retort, I was obliged to interrupt the

the operation. I had no better success when the sulphur, previous to its distillation, had been moistened with marine acid.

Again, I exposed 18 grains of liver of sulphur to six cubic inches of *fixed air*, thermometer 70°, for four days. The liver of sulphur was somewhat whitened on the surface; the air had not an hepatic smell, but rather that of bread. It was not converted into phlogisticated air, but seemed to have taken up some sulphur, which lime-water separated. It was not in the least diminished, and therefore seems to have received an addition of hepatic air, or rather of sulphur.

I also exposed a quantity of *sulphureo-martial paste* to fixed air, for five days. The fixed air was not diminished, but received a slight accession of inflammable air. The paste itself, taken out of this air, and exposed to the atmosphere, heated strongly.

Lastly, I exposed 3 grains of sulphur to about 12 cubic inches of *marine air*. It was not diminished in four days; nor was the sulphur sensibly. On adding one cubic inch of water to this air, it was absorbed all to one inch, and this had an hepatic smell; so that neither was the sulphur decomposed, nor the marine acid converted into inflammable air. The water had also an hepatic smell, and evidently contained sulphur; for it precipitated the solution of silver *brown* mixed with white, and the nitrous solution of copper *reddish brown*, and when vegetable fixed alkali was dropped into it, let fall a white precipitate, namely, the sulphur.

SECTION II.

Of the general Characters of Hepatic Air.

I found the absolute weight of this air by weighing it in a glass bottle, previously exhausted by Mr. HURTER's new improved air-pump, whose effect is so considerable as to leave in general only $\frac{1}{1000}$ and frequently but $\frac{1}{10000}$ part of unexhausted air. This bottle contained 116 cubic inches nearly; and this quantity of hepatic air weighed 38,58 grains, the thermometer being then $67^{\circ},5$, the barometer 29,94, and M. SAUSSURE's hygrometer 84° , the weight of 116 cubic inches of atmospheric air being at the same time 34,87 grains; hence a cubic foot of hepatic air weighs, in these circumstances, 574,7089 grains, and 100 cubic inches of it weigh about 33 grains; and its weight, relatively to that of common air, is as 10000 to 9038 *. This hepatic air was extracted from artificial pyrites by marine acid.

The inflammability of this air has been already mentioned. It never detonates with common air; nor can it be fired, in a narrow-mouthed vessel, unless mixed with a considerable proportion of this air. M. SCHEELÉ found it to inflame when mixed with two-thirds of this air. According to M. SENNEBIER,

* Hence the weight which I have assigned to common air in my first paper, after M. FONTANA, is evidently erroneous; and, indeed, by that determination common air would not be even 700 times lighter than water, in the temperature of 55° , and the barometer 29,5, which contradicts all barometrical and aerostatic experiments: and I cannot omit mentioning the very near agreement of the weight of common air here found with that resulting from the calculation of Sir GEORGE SHUCKBURY, it is so great as to differ only by 2 grains in a cubic foot.

it

it cannot be fired by the electric spark, even when mixed with any proportion of respirable air. I found a mixture of one part of hepatic air and 1,5 of common air to burn blue, without flashing or detonating. During the combustion sulphur is constantly deposited, and a smell of vitriolic air is perceived. A mixture of half hepatic and half nitrous air burns with a bluish, green, and yellow lambent flame; sulphur is also deposited, and in proportion as this is formed, a candle dipped in this air burns more weakly, and is at last extinguished. A mixture of two parts nitrous and one of hepatic air partially burns, with a green flame, and a candle is extinguished in the residuum, which then reddens on coming in contact with atmospheric air. I made a mixture of one part nitrous and one part hepatic air, and to this admitted one part also of common air; the instant the common air was introduced, sulphur was precipitated, and the three measures occupied the space of 2,4 measures; this burned on the surface with a wide greenish flame, but the candle was extinguished when sunk deeper.

A mixture of four parts common air and one part hepatic burned blue and rapidly; but a mixture of one part dephlogisticated and one part hepatic, which had stood eight days, went off with a report as loud as that of a pistol, and so instantaneously that the colour of the flame could scarce be discerned.

Every species of hepatic air turns the *tincture of litmus* red. M. BERGMAN seems to think, that, if this air were washed, it would not produce this effect; yet, when I had passed two measures of it through one of water, or when I had boiled it out of water impregnated with it, or even when I passed that which had already reddened litmus, into a fresh quantity of litmus, it still preserved the same property, which I therefore consider as essential to it.

With

With respect to *solubility in water*, hepatic airs extracted from different materials differ considerably. In the temperature of 66° , water dissolves, by slight agitation, two-thirds of its bulk of alkaline or calcareous hepatic air, extracted by marine acid; three-fourths of its bulk of martial hepatic air, extracted by the same acid; eight-tenths of that extracted by means of the concentrated vitriolic acid, or the dilute nitrous or saccharine acids in the temperature of 60° ; seven-tenths of sedative hepatic air; nine-tenths of acetous hepatic air, and of that afforded by oil of olives; and its own bulk of that produced from a mixture of sugar and sulphur. In général, I imagined that which required most heat for its production to be most soluble: though in some instances, particularly that of acetous hepatic air, that circumstance does not take place.

But the most remarkable phænomenon attending the union of hepatic air with water is, that it is not permanent. Even water, out of which its own air had been boiled, in a few days after saturation with hepatic air grows turbid, and in a few weeks deposits most of it in the form of sulphur, though the bottle be ever so well stopped, or stand inverted in water or mercury. Yet water no way decomposes hepatic air by absorbing it; for the part left unabsorbed by a quantity of water is absorbable by a larger quantity of water, and burns like other hepatic air. Heat does not expel this air from water, until carried to the degree of ebullition.

No species of hepatic air, which I have examined, precipitates lime from *lime-water*, except the carbonaceous; and even this scarcely produces a sensible precipitation, unless a large quantity of it be made to pass through a small quantity of lime-water.

The solution of *acetous baro-selenite*, (that is, ponderous earth dissolved in distilled vinegar) is rendered brown and turbid by this air, but that of *marine baro-selenite* is not altered; nor are the solutions of other earths. Metallic solutions are affected by it in the same manner as by hepatic water, of which I shall treat in the fifth section.

But of all the tests of hepatic air, the most delicate and sensible is the *solution of silver in the nitrous acid*. This, according as the nitrous acid is more or less saturated with silver, becomes black, brown, or reddish brown, by contact with hepatic air however mixed with any other air or substance. When the acid is not saturated, or is in large proportion, the brown or black precipitate, which is nothing but sulphurated silver, is re-dissolved.

It should also be remarked, that all hepatic air is somewhat diminished by long standing on mercury, whose surface is then blackened by it. This is particularly the case of carbonaceous hepatic air, which certainly carries over and volatilizes part of the charcoal from which it is extracted, especially that portion of air which comes over in the greatest heat; this it deposits on the addition of water.

S E C T I O N III.

Of the Action of Hepatic and other Aerial Fluids on each other.

Six cubic inches of common and six of hepatic air being mixed with each other, and standing over mercury for eight days, were not in the least altered in their dimensions or otherwise; though a diminution of a $\frac{1}{110}$ th part might be perceived. The mercury was slightly blackened. The event was the same when

when three measures of common and one of hepatic air were used. Water took up the hepatic air. No fixed air was found.

Five measures of hepatic, and five of *dephlogisticated* air so pure that one measure of it and two of nitrous air made only three-tenths of a measure, remained unaltered for eight days, the mercury only being blackened. No fixed air was produced, nor the dephlogisticated air phlogisticated. When the mixture was fired, it went off all at once with a loud report.

Four measures of phlogisticated and four of hepatic air remained undiminished for sixteen days: water then took up the hepatic, and left the phlogisticated air.

Four measures of *inflammable* and four of hepatic air remained unaltered for six days.

Two measures of hepatic and two of *marine acid* air suffered no diminution in three days. The mercury on which they stood was not blackened. Water took up both, and precipitated the solution of silver black.

The same quantity of hepatic and *fixed air* remained four days without any sensible diminution. Four measures of water absorbed the greater part of both, had an hepatic smell, precipitated lime from its solution, and also silver, as usual. The residuum extinguished a candle.

But *vitriolic*, *nitrous*, and *alkaline* airs had very remarkable effects on hepatic air.

Two measures of hepatic being introduced to two of *vitriolic* air, a whitish yellow deposition immediately covered both the top and sides of the jar, and both airs were, without any agitation, reduced to little more than one measure; but the opacity of the incrustated glass prevented my then ascertaining the diminution with precision. Hence I repeated this experiment more at large, in the following manner. To five measures of *vitriolic* air (each measure containing a cubic inch) I added one

of hepatic air. In less than a minute, without any agitation, the sides of the glass were covered with a whitish scum, which seemed moist, and a diminution took place of more than one measure. In four hours after, I introduced a second measure of hepatic air, which was followed by a similar diminution and deposit. The next day I added three more measures of this last, at the interval of four hours between each; and still finding a considerable diminution after each, I the following day added another measure; the diminution produced by this last appeared to me not to exceed one measure. I then poured off the residuary air into another jar, and found it not to exceed three measures; so that here eleven measures, namely, five of vitriolic and six of hepatic air, were reduced to three. Into one measure of this residuary air I introduced a lighted candle: it was immediately quenched. To the two remaining measures I added one measure of water: by agitation it took up four-tenths of its bulk. To part of the remainder I added nitrous air, which had no effect upon it. Another part of it extinguished a candle. It had not a vitriolic smell.

The water which had taken up four-tenths of its bulk of this air did not precipitate lime; nor did it affect acetous baroselenite in less than a quarter of an hour, and then produced a very slight cloud. It sensibly reddened litmus, and precipitated the solution of silver white; and hence it appears to have taken up a very minute portion of vitriolic acid. And what was not taken up by water seems to have been mere phlogisticated air.

I afterwards washed the sulphur, which coated the jar, with distilled water. This water slightly reddened litmus, precipitated not only the acetous, but also marine baroselenite copiously, as well as marine and nitrous selenite; also the nitrous solutions of silver, lead, and mercury, all white. It even precipitated

precipitated lime from lime-water, forming a cloud in it, which neither the fixed nor volatile acid of vitriol can produce. Hence this water contained nothing hepatic; but, on the contrary, a considerable proportion of the aërial and vitriolic acids*.

With *nitrous air* I made the following experiments. First, I found that two measures or cubic inches of nitrous and two of hepatic air were little altered when first mixed, even by agitation; but after thirty-six hours both were reduced to nearly one-third of the whole, but something more. Yellow particles of sulphur were deposited both on the mercury, and on the sides of the jar, but the mercury was not blackened. The residuary air had still an hepatic smell, and was somewhat further diminished by water; and in the unabSORbed part a candle burned naturally. The water had all the properties of hepatic water.

Perceiving by this experiment that I had not employed enough of nitrous air to condense the hepatic perfectly, to eight cubic inches of hepatic air I added nine of nitrous air, all at once; a yellowish cloud instantly appeared, a slight white scum was deposited on the sides of the jar, and the whole seemed diminished about two cubic inches, or between one-ninth and one-eighth, the temperature of the room being then 72°. I then laid by the mixture, and in forty-eight hours after, I found the whole reduced to six cubic inches, and the top and sides of the jar covered with a white cake of sulphur, the heat of the room being constantly kept between 60° and 70°. Finding the diminution to reach no further in

* Note, the vitriolic acid air here employed was the purest possible; it was extracted from sulphur distilled with precipitate *per se*.

twenty-four hours more, I examined the residuary air. It exhibited the following appearances.

1°, It had the smell of alkaline air pretty strongly; at least that smell issued from the jar that contained it after the air itself was poured into another jar.

2°, A candle burned in it naturally.

3°, It did not affect tincture of litmus or lime-water, or acetic barytes.

4°, No species of air had any effect on it except the dephlogisticated, with which it produced a slight redness and diminution.

5°, It produced a slight white precipitate in solution of silver.

It is plain, this air is the same as that which Dr. PRIESTLEY calls *dephlogisticated nitrous air*, and which, I think, may more properly be called *deacidified nitrous air*. A further examination of it would lead me too far from the present subject: I shall therefore defer it until another opportunity.

As it appeared to me, from the experiment mentioned in the second section (in which I found sulphur precipitated from a mixture of nitrous and hepatic air, immediately after the admission of common air) that an uncombined acid in the nitrous air was the cause of the precipitation of sulphur; I attempted depriving nitrous air of any loose acid it might contain, before I should mix it with hepatic air.

1st, I made some nitrous air from silver very carefully over boiled and filtered water, and found it to contain an acid, for it strongly reddened tincture of litmus.

2dly, I admitted alkaline air to this nitrous air until it no longer caused any cloud, and then washed out the ammoniacal compound in distilled water; after which I transferred this

purified nitrous air to the mercurial tub. It appeared to lose, by privation of its acid, about one-sixth of its bulk; and it was diminished by common air just in the same manner as unpurified nitrous air is.

Then to 8 cubic inches of this purified nitrous air I admitted all at once 7 cubic inches of hepatic air. No cloud, diminution, or deposit, appeared; but in six hours after (the temperature of the room being all the time at 76°), the whole was reduced to 5 cubic inches; the diminution went no further eighteen hours after. Sulphur, much whiter than in the former experiments, was deposited, and both in this and in the former experiments that part of it which, by the rising of the mercury, was intercepted between it and the jar, was of a yellow and red shining colour, and not black as that deposited on mercury usually is. The residuary air flashed with so much vehemence as to extinguish the candle dipped into it, by the violence of the blast. The flame was exceeding white and vivid; but it did not detonate in the least, but rather resembled dephlogisticated air. The jar out of which it had been transferred had a sharp alkaline smell.

This air was not in the least diminished by nitrous air, even when heated to 150 degrees; which heat I contrived to produce by passing the upper part of the jar that contained this air into another wider jar, furnished with a perforated cork bottom, and filling this with water heated to that degree.

Water poured into the jar in which the sulphur was deposited, produced a bluish white cloud in solution of silver, though insipid to the taste.

Hence it appears to me, that, whatever this air may be, it had been deacidified by hepatic air still more perfectly than
that

that in which a candle burns naturally; and that it is by no means dephlogisticated.

Lastly, *Alkaline* and hepatic airs, perfectly pure and mixed in proper proportions, would probably destroy each other completely, though I have not been able to effect this intirely. Six measures of hepatic air from liver of sulphur and 6 of alkaline air immediately throw up a white cloud, leave a whitish scum on the sides of the jar, and are reduced to about 1 measure. On adding water this is reduced to about one-half; and in this I found a candle to burn naturally: but the following experiments, being made with more care, prove that this residuary air was only the common air of the vessels.

To 6 cubic inches of calcareous hepatic air I admitted, all at once, 7 of alkaline air; a white cloud and a little white scum at first appeared; but in a few seconds the whole was reduced to six-sevenths of a cubic inch; and on adding 2 measures of water, only one-ninth of a cubic inch of air remained. This could not be inflamed. The water, thus impregnated, precipitated a solution of silver black. In this experiment great care was taken to have each of the mixed airs as pure as possible, and the alkaline was admitted all at once, instead of by different portions, merely with that view; and it is probable, that, if the due proportion were hit upon, nothing would remain. The scum appears to be almost liquid, and as soon as the jar is emptied of mercury, it breaks out into a white smoke, with an exceeding sharp urinous smell.

Five measures of martial hepatic air were, upon the admission of $5\frac{1}{4}$ of alkaline air, reduced to something more than one measure, and upon the addition of water there remained but half a measure; and this was inflammable, with detonation; the
inflammable

inflammable air undoubtedly proceeding from the solution of the iron.

Five cubic inches of saccharine hepatic air, mixed with 5 of alkaline air, were diminished more slowly; for after five minutes there still remained 4,5 cubic inches. I then added another measure of alkaline air: in three hours after there remained but 1,25 cubic inches. In passing this residuum through water it was reduced to about half a cubic inch; and this burned with a blue lambent flame, without leaving a vitriolic smell or any deposit on the glass; so that it clearly was inflammable air from the sugar.

I once imagined I had obtained inflammable air from a mixture of alkaline air with hepatic air drawn from liver of sulphur; but I afterwards found this inflammable air proceeded from a very slight contamination of zinc in the mercury over which my airs had been produced; the alkaline air acted on this zinc, and must have produced the inflammable air; for when I afterwards received and mixed these airs over mercury, perfectly purified, I obtained no more inflammable air.

SECTION IV.

Of the Action of Hepatic Air, and Acid, Alkaline, and Inflammable Liquids, on each other.

One measure of *oil of vitriol*, whose specific gravity was 1,863, absorbed two measures of hepatic air all to one-tenth. The acid was whitened by a copious deposition of sulphur. I also introduced, over mercury, a measure of red *nitrous acid*, whose specific gravity was 1,430, to an equal measure of hepatic air; red

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vapours instantly arose, and only one-tenth or one-twelfth of a measure remained in an aërial form; but as the acid acted on the mercury, I was obliged to carry the jar into the water tub, by which means the whole was absorbed: no sulphur was here precipitated.

I repeated this experiment in another manner. Having produced 4,5 measures of hepatic air over mercury, I transferred them to the water tub, and instantly by means of a syphon blew into them one measure of the above concentrated nitrous acid; but though I managed as quickly as possible, the hepatic air was something diminished by contact with the water, before the acid had entered the tube that contained the air. I then stopped the tube with a ground glass stopper, and laid it by for twelve hours; after which interval I found the liquor in the tube white and turbid, and but weakly acid, much water having entered in spite of my endeavours to exclude it. The remaining air slightly detonated on presenting to it a lighted candle, and had an hepatic smell. But as this hepatic air was obtained from sulphureo-martial paste, it does not prove that inflammable air enters into the composition of other hepatic airs, derived from the union of sulphur with substances that do not yield inflammable air.

Finding it so difficult to subject hepatic air to the direct action of the concentrated nitrous acid, I diluted it to that precise degree at which it could not act on mercury without the assistance of heat, and then passed through it an equal bulk of the same hepatic air; the acid was whitened, and eight-tenths of the air absorbed, and the residuum detonated. Repeating the same experiment with hepatic air from liver of sulphur, I found still more of it absorbed by the acid; but the residuum

no

no longer detonated, but burned with a blue and greenish flame, and sulphur was deposited on the sides of the jar.

Observing this dilute acid to absorb nearly three times its bulk of alkaline hepatic air, I expelled this air from it by heat, but obtained only one-sixth of the air that had been absorbed; and in this a candle burned naturally.

Two measures of alkaline hepatic air, being exposed to one of strong *marine acid*, were absorbed, by slight agitation, all to one-fifth of a measure. A third measure of air being then added, there remained, after some agitation, but half a measure. Sulphur was precipitated as usual; but the mercury over which the acid stood attracted it from the acid; for it was blackened, which did not happen when the former acids were used. The residuum burned just as pure hepatic air.

Distilled vinegar absorbs nearly its own bulk of air, and becomes slightly whitened; but by agitation it may be made to take up about twice its bulk, and then becomes very turbid.

One measure of *caustic vegetable alkali*, whose specific gravity was 1,043, absorbed nearly four measures of alkaline hepatic air. It was at first rendered brown by it; but after some time it grew clear, sulphur was deposited, and the surface of the mercury blackened. This shews that alkalies are not dephlogisticated by silver or other metals, as Mr. BAUME imagined, but only cleared of part of the sulphur, which they commonly contain, it being formed by the tartar vitriolate contained in the plant, and coal, during combustion.

One measure of *caustic volatile alkali*, whose specific gravity was 0,9387, absorbed 18 of hepatic air. If the caustic liquor contained more alkali, it would absorb more hepatic air, as 6 measures of hepatic unite to 7 of alkaline air; and thus the strength of alkaline liquors, and their real contents, may be

determined better than by any other method. Also the smoaking liquor of BOYLE, which is difficultly prepared in the usual way, may easily be formed by placing the volatile alkali in the middle glass of Dr. NOOTH's apparatus for making artificial mineral waters, and decomposing artificial pyrites, or liver of sulphur, in the lower glass, by marine acid.

Oil of olives absorbs nearly its own bulk of this air, and obtains a greenish tinge from it.

But *new milk* scarcely absorbs one-tenth of its bulk of this air, which is very remarkable, and is not in the least coagulated.

Oil of turpentine also absorbs its own bulk of this air, and even more; but then becomes turbid. Water seems also to precipitate this air from it, for when shaken with it a white cloud appears.

Spirit of wine, whose specific gravity was 0,835, absorbed nearly three times its bulk of this air, and became brown. By this means sulphur may be combined with spirit of wine much more easily than by Count LAURAGAIS' method, the only hitherto known. Water precipitates the sulphur in part.

Sulphurated spirit of wine does not tinge litmus red; but it precipitates lime-water, as highly rectified spirit of wine singly does. It also precipitates and gives a brown colour to acetous baro-selenite, which spirit of wine alone also does. It turns the solution of silver black and reddish brown. Concentrated vitriolic acid precipitates the sulphur from it, which neither the nitrous nor marine acids can effect.

When hepatic air is mixed with an equal bulk of *vitriolic æther*, the bulk of the air is at first increased; but afterwards half of it is absorbed, and a slight precipitation appears. The smell of the æther is mixed with that of the hepatic air; but on adding water

water it becomes very offensive, resembling that of putrefying animal substances.

To one measure of hepatic air I added 1,5 of the *nitrous solution of silver*: the air was immediately, without agitation, reduced to half a measure, and the solution blackened. The remaining air admitted a candle to burn naturally. Hepatic air was also absorbed, but not so readily, nor in such quantity, by the solution of *vitriols of iron and silver*; that of silver was blackened; that of iron at first became white, but by agitation darker. The residuary air burned blue, as hepatic air usually does.

SECTION V.

Of the Properties of Water saturated with Hepatic Air.

This water turns tincture of litmus red.

It does not affect lime-water.

It does not form a cloud in the solution of marine, though it does in that of acetous baro-selenite.

The solutions of other earths in the mineral acids are not altered by it.

When dropped into a solution of *vitriol of iron* or marine salt of *iron*, it produces a white precipitate.

In nitrous salt of *copper* it causes a brown precipitate, and the liquor is changed from blue to green. The precipitate redissolves by agitation. In vitriol of copper it forms a black precipitate.

The solution of *tin* in aqua regia is precipitated by it of a yellowish white colour; that of *gold*, black; that of *regulus of antimony*,

antimony, red and yellow; that of *platina*, red mixed with white.

The solution of *silver* in the nitrous acid, and also that of *lead*, whether in the acetous or nitrous acid, are precipitated black. If the solutions are not perfectly saturated with metal, the precipitates will be brown or reddish brown, and may be redissolved by agitation.

The nitrous solution of *mercury* is precipitated of a yellowish brown; that of sublimate corrosive, yellow mixed with black; but by agitation it becomes white.

The nitrous solution of *bismuth* becomes, by mixture with this water, reddish brown, and even assumes a metallic appearance; that of *cobalt* becomes dark; that of *zinc*, of a dirty white; that of *arsenic*, in the same acid, yellow mixed with red and white, orpiment and realgar being formed.

If *oil of vitriol*, whose specific gravity is 1,863, be dropped into hepatised water, it renders it slightly turbid; but, if the *volatile vitriolic acid* be dropped into it, a bluish white and much denser cloud is formed in the water.

Strong nitrous acid, whether phlogisticated or not, causes a copious white precipitation; but *dilute* nitrous acid produces no change. *Green* nitrous acid, whose specific gravity was 1,328, immediately precipitated sulphur from it.

Strong marine acid produced a slight cloud; but neither distilled vinegar nor acid of sugar had any effect.

It is said by Mr. BERGMAN, that hepatised water in a well closed vessel effects a solution of iron in a few days; but this experiment, on repeated trials, did not succeed with me: nor could I dissolve any other metal in this water; the sulphur indeed unites to many of them, but forms an insoluble mass;

so that, I presume, metallic substances can never be found in hepatized mineral waters.

SECTION VI.

Of the Properties of Alkaline Liquors impregnated with Hepatic Air.

I have already mentioned the proportion of air they are able to take up. Colourless fixed alkaline liquors receive a brownish tinge from this air. The residuum they leave is of the same nature as the part they absorb.

A caustic fixed alkaline liquor, saturated with this air, precipitates *barytes* from the acetous acid, of a yellowish white colour. It also decomposes other earthy solutions, and the colour of the precipitates varies according to their purity, and perhaps this test might be so far improved as to supply the place of the Prussian alkali.

It precipitates the solution of nitriol of iron, and also marine salt of iron, black; but this latter generally whitens by agitation. That which I used was very saturated.

The solutions of silver and lead are also precipitated black with some mixture of white; that of gold is also blackened; but that of platina becomes brown.

Solutions of copper let fall a reddish black or brown precipitate.

Sublimate corrosive by this test discovers a precipitate partly white and black, and partly orange and greenish.

In the nitrous solution of arsenic it forms a yellow and orange; and in that of regulus of antimony, in aqua regia, an orange precipitate mixed with black.

Nitrous.

Nitrous solution of *zinc*, thus treated, shews a dirty white; that of *bismuth* a brown mixed with white; and that of *cobalt* a brown and black precipitate.

As *Prussian alkali* always contains some iron, it gives a purple precipitate with this test, which precipitate is easily dissolved.

It turns tincture of raddishes, which is my test for alkalies, green.

The action of liver of sulphur on metallic substances in the *dry way* is described by many authors, and particularly in an excellent Dissertation by M. ENGESTROM; but its action in the *moist way* has not been mentioned, as far as I recollect, by any. Hence I tried its effect on a few grains of iron, copper, lead, tin, zinc, bismuth, regulus of antimony, and of arsenic. Putting each into a bottle, containing about three half ounces of liquid liver of sulphur, so far diluted that its colour was yellow; in about fifteen days I found they all, except the zinc and tin, had attracted sulphur from the fixed alkali. Iron, arsenic, and regulus of antimony and lead, were most altered; copper next, and bismuth least: but the liquors held none of the metals in solution; that which contained iron became green; on adding an acid sulphur was precipitated; if it held iron it could not at that period be detected.

Water saturated with the condensed residuum of alkaline and hepatic air, that is, with the purest volatile liver of sulphur, does not precipitate marine *selenite*, though it forms a slight brown and white cloud in that of marine *baro-selenite*.

It produces a black precipitate in the solution of vitriol of *iron*, and a black and white in that of marine salt of iron; but by agitation this last becomes wholly white.

It

It precipitates both vitriol of copper, and the nitrous salt of copper, red and brown.

Tin in aqua regia gives a yellowish precipitate; *gold* a dilute yellow and reddish brown; *platina* a flesh-coloured precipitate; and *regulus of antimony* a yellowish red.

Silver is precipitated black; and so is *lead* both from the nitrous and acetous acids.

Sublimate corrosive appeared for an instant red; but soon after its precipitate appeared partly black and partly white.

The nitrous solution of *bismuth* affords also a precipitate, partly black, partly white, and partly reddish brown, and of a metallic appearance; that of *cobalt* is also black or deep brown.

Arsenical solutions give yellow precipitates more or less red; but those of *zinc* only a dirty white.

All these colours vary in some degree, according as the liquors are more or less saturated previous to and after their mixture, and the time they have stood together.

SECTION VII.

Of the Constitution of Hepatic Air.

From an attentive consideration of the above experiments, which I purposely disengaged from all theory, it is difficult to conclude, that hepatic air consists of any thing else than sulphur itself, kept in an aerial state by the matter of heat. Every attempt to extract inflammable air from hepatic air, when drawn from materials that previously contained nothing inflammable, namely, from alkaline or calcareous hepars,

proved abortive : on the contrary, when the materials could previously supply inflammable air, as when martial carbonaceous and saccharine compounds were employed, inflammable air, in ever so small a proportion, was detected : nor could hepatic air be procured from the direct union of inflammable air and sulphur, as we have seen.

Some have imagined, that this air consists of liver of sulphur itself volatilized, and consequently that an alkali enters into its composition ; but many weighty reasons oppose this supposition. In the first place this air is evidently, though weakly acid, since it reddens litmus, and precipitates acetous baro-selenite. 2dly, It may be extracted from materials that either contain no alkali at all, or next to none, as iron, sugar, oil, charcoal : and, lastly, it is not decomposed by marine or fixed air, by which, nevertheless, liver of sulphur is decomposable.

I formerly thought that sulphur was held in solution in hepatic air, either by vitriolic or marine air ; yet though both of them may hold sulphur in solution, as we have seen, still neither of them is essential to the constitution of hepatic air as such, since it is producible from materials that contain neither of these acids ; and, from whatever subject it is obtained, it exhibits the characters of one and the same acid, namely, the *vitriolic exceedingly weakened* ; and such an acid we may suppose sulphur itself to be.

In effect, sulphur, even in its concrete state, affords many characters of acidity. It unites with alkalies, calcareous and ponderous earths, and most metals, as a weak acid might : and except a manifest solubility in water (a property which some other concrete acids also possess in a very weak degree) it exhibits every character of acidity. But its acidity is the weakest possible, since it decomposes only acetous, and not marine baro-selenite,

selenite, and is separable from alkalies and earths by all other acids.

That the matter of heat enters into the composition of this air, is evident from the experiments of M. SCHEELÉ, who paid particular attention to that object. He found, that acids excite much less sensible heat in uniting with *liver of sulphur*, whether alkaline or calcareous, than while uniting with a proportion of caustic fixed alkali or lime equal to that which enters into the composition of those *livers*; whence he justly infers, that the *difference* enters into the composition of the hepatic air produced. I have proved the same thing another way: for, instead of decomposing an *alkaline* hepar by marine acid, I tried to decompose it by a saturate solution both of marine selenite and marine Epsom. The decomposition indeed took place, but no hepatic air was produced: for the acid having given out its specific heat on uniting to the earths, had none to lose or communicate on uniting to the alkali, and consequently the sulphur receiving none could not be thrown into an aerial state.

It is remarkable, that bodies capable of an aerial form receive the latent heat necessary for that form, much more readily from a body that parts with its specific heat than by the mere application of sensible heat. Thus aerated barytes cannot be decomposed by mere heat, as Dr. WITHERING has shewn, though its air is easily separated from it by an acid; and in the same manner antimony cannot be desulphurated even by vitrefaction, though it may by acids: so liver of sulphur will not give hepatic air by mere heat, though it will by the intervention of an acid, even the weakest. The reason of which seems to be this: the matter of heat has no particular affinity with any substance, as is evident from its passing

indifferently from any hot body to a colder, of whatever sort or kind the bodies may be; but it is determined to unite with this or that body in a latent state, in greater or lesser quantity, in proportion to the greater or lesser *capacity* of these bodies to receive it. Now acids, by uniting to the alkaline basis of liver of sulphur, expel the sulphur, and give it their heat, at the *instant* the sulphur, by its separation, has the capacity to receive it; whereas sensible external heat, acting alike on both the constituent parts of liver of sulphur, separates neither; or if it separates them, yet, by its *successive* action, it throws one of them into a *vaporous* state first, and bodies that *first* acquire this state can never after acquire an aerial state by any *subsequent* accession of heat.

The vitriolic and nitrous acids are less adapted to the production of hepatic air than the marine acid, though they contain more specific heat than the mere acid part of the marine acid: the most probable reason of which is, because they have a stronger attraction to sulphur itself, and so detain it.

Hepatic air is much disposed to give out its latent heat, particularly when in contact with substances to which it has any affinity; thus it is condensed in water in a few days; it is also condensed by long exposure to fresh surfaces of mercury or silver or other metals, particularly if they are moist. MR. BERGMAN found it in great measure condensed into sulphur, when inclosed alone in a bottle*. In this case it probably contained an excess of sulphur; for hot hepatic air is capable of keeping a farther quantity of sulphur in solution, and depositing it when cold, as I have frequently observed.

* See a note in the second volume of M. MORVÉL'S translation of the second volume of BERGMAN'S Works, p. 341.

The precipitation of metallic substances by this air is owing partly to the union and phlogistication of the acids by this air, and partly to its union with the metals themselves, for it evidently unites in most cases to both.

As alkalies and sulphur are known to have an affinity to each other, we easily understand why hepatic and alkaline airs are condensed when mixed with each other; nor is there any difficulty in conceiving why hepatic air is not condensed by common, dephlogisticated, inflammable, or phlogisticated airs, or remarkably by marine air; but it seems very extraordinary, that hepatic air and vitriolic air should be condensed, and in great measure converted into sulphur by their mutual action on each other, particularly as they both seem nearly of the same species, or at least nearly allied to each other. The attraction of two bodies, thus circumstanced, is certainly very extraordinary; yet that their union proceeds from attraction seems pretty evident, for concentrated vitriolic acid, and particularly volatile vitriolic acid, precipitates sulphur copiously from hepatised water. Volatile vitriolic acid frequently holds some sulphur in solution, as appears from the experiments of Dr. PRIESTLEY and M. BERTHOLLET; and hence it deposits some when it loses its aerial form, or by mere length of time; but the whole of this condensed air is not turned into sulphur, for the water that washed the precipitated sulphur took up a quantity of volatile acid and fixed air, as has been shewn.

The condensation of hepatic air by nitrous air seems owing to the same cause; for when the nitrous air was in great degree deprived of superfluous acid, the condensation of the hepatic was much slower; and that which after all took place seems to have been effected by the decomposition of the nitrous air, and the consequent extrication of an acid.

The

The decompositions effected by fixed and volatile livers of sulphur obviously proceed in most cases from a double affinity.

S E C T I O N VIII.

Of Phosphoric Hepatic Air.

As phosphorus, in respect to its constituent parts, bears a strong resemblance to sulphur, I was naturally led to examine its phenomena when placed in the same circumstances: I therefore gently heated about 10 or 12 grains of phosphorus, mixed with about half an ounce of caustic fixed alkaline solution, in a very small phial, furnished with a bent tube, and received the air over mercury. Upon the first application of heat two small explosions took place, attended with a yellow flame and white smoke, which penetrated through the mercury into the receiver; these were followed by an equable production of air. At last the phosphorus began to swell and froth, and fearing the rupture of the phial, I stopped the tube to prevent the access of atmospheric air, and removed the phial to a water tub, intending to throw it in; but in the mean while the phial burst with a loud explosion, by reason of an obstruction in the tube, and a fierce flame immediately issued from it. However I obtained about 8 cubic inches of air.

This air was diminished very slightly, by agitation with an equal bulk of water, and then became cloudy like white smoke, but soon after recovered its transparency. Upon turning up the mouth of the tube to examine the water, the unabsorbed air instantly took fire, and burned with a yellow flame without exploding, leaving a reddish deposit on the sides of the tube.

Water

Water impregnated with phosphoric air, and over which this air had burned, slightly reddened tincture of litmus :

Did not affect Prussian alkali :

Had no effect on the nitrous solutions of *copper* or *lead*, *zinc* or *cobalt*, nor on marine solution of *iron* or *tin*, or of tin in aqua regia, nor on the vitriolic solutions of *iron*, *copper*, *tin*, *lead*, *zinc*, *regulus of antimony*, *arsenic*, or *manganese* ; nor on the marine solutions of *iron*, *copper*, *lead*, *zinc*, *cobalt*, *arsenic*, or *manganese*.

But it precipitated the nitrous solution of *silver* black, and vitriol of silver brown ; also nitrous solution of *mercury* made without heat brown and black ; but vitriol of mercury first became reddish, and afterwards white ; and sublimate corrosive yellow and red mixed with white.

Gold dissolved in aqua regia is precipitated purplish black, and from the vitriolic acid brownish red and black ; but *regulus of antimony* in aqua regia is precipitated white by this phosphorated water.

The nitrous solution of *bismuth* shewed first a white, and presently after a brown precipitate. Vitriol of bismuth and marine salt of bismuth were also precipitated brown ; this latter re-dissolved by agitation.

The nitrous solution of *arsenic* also became brown, but re-dissolved by agitation.

I afterwards impregnated some water with this air, without suffering the air to burn over it : it scarcely affected litmus, did not precipitate lime-water ; but it caused a black precipitate in solution of *silver*, a white precipitate of *regulus of antimony* in *R.*, and a whitish yellow in that of *sublimate corrosive*.

To a measure of this air I let up a measure of water, and through this some small bubbles of *common air* ; every bubble flamed.

flamed and produced a white smoke until about half as much common air was introduced as there was originally of phosphoric; and yet the original bulk did not appear increased; the flame each time produced a small commotion, and a smoke descended after inflammation into the water: when flame ceased to be produced, smoke still followed the introduction of more common air. Bubbles of phosphoric air, escaping through mercury into the atmosphere, flame, crackle, and smell, exactly like the electric spark *.

To a measure of phosphoric air I let up a half measure of *nitrous air*: a white smoke appeared, with an exceeding slight diminution, and the transparency was soon restored, a slight scum being deposited on the sides of the jar. Another half measure of nitrous air produced no smoke or diminution; but on adding water, and agitating the air in it, much more of it was absorbed. Upon turning up the jar the nitrous air first escaped in the form of a red vapour, and this was followed by a whitish smoke. The water had a phosphoric smell, and precipitated the solution of silver brown. In this experiment the acid of the nitrous air seems to have acted the same part that it does in hepatic air.

Phosphoric air was scarce at all diminished by the addition of an equal measure of *alkaline air*; and water being put up to these, took up in appearance little else than alkaline air, yet on turning up the mouth of the jar, the residuary air smoked without flaming.

* A few months after I made these experiments on phosphoric air, the tenth volume of the *Mémoires des Savans Etrangers* was published; and in this I found, that the spontaneous inflammation of this air was known to M. GREGGEMBE in the year 1783. His experiments are now published in ROSSER's *Journal* for October, 1785.

The

The water, thus impregnated, had exactly the smell of onions. It turned tincture of radishes green.

It precipitated solution of *silver* black; and that of *copper* in the nitrous acid brown; but this precipitate was re-dissolved by agitation, and the liquor became green. *Sublimate corrosive* was precipitated yellow mixed with black.

Iron was precipitated white, both from the vitriolic and marine acids; but a pale yellow solution of it in the nitrous acid was not affected; and a red solution of it in the same acid was only conglutinated.

Regulus of antimony in aqua regia gave a white, *cobalt* in nitrous acid a very slight reddish, and *bismuth* in the same acid a brown precipitate.

But neither the nitrous solution of *lead* or *zinc*, nor that of *tin* in marine acid or aqua regia, nor that of *regulus of antimony* in aqua regia, were any way affected.

Fixed air, mixed with an equal proportion of phosphoric air, produced a white smoke, some diminution, and a yellow deposit. On agitating the mixture in water, the fixed air was taken up all to one-tenth. The residuum smoked, but did not inflame spontaneously.

To a small portion of phosphoric air I introduced some *precipitate per se*. It soon grew black, and a white smoke appeared. In two days the precipitate remained solid, yet acquired a shining pale white colour, like that of steel: the air lost its spontaneous inflammability; but I am not certain that this want of inflammability did not proceed from some other cause; for two days after I made this air, I found a quantity of it, which had rested all night on water, had deposited a yellow scum on the sides of the jar, and lost its spontaneous inflammability next morning. The temperature of the air was

then 53° ; and when it inflamed before, the temperature was 68° .

From these few experiments, which the small quantity of air I then obtained did not suffer me to repeat, I think we may conclude, that phosphoric air is nothing else but phosphorus itself in an aërial state, and differs from sulphur in this, among other points, that it requires much less latent heat to throw it into an aërial form, and hence may be disengaged from fixed alkalies, without the assistance of an acid.

VI. *Observations on the Affinities of Substances in Spirit of Wine.*
In a Letter to Richard Kirwan, Esq. F. R. S. by John
Elliot, M. D.

Read January 19, 1786.

S I R,

IN your excellent papers on the attractive powers of the mineral acids, you shew that metallic calces have stronger attractions to those acids, than alkalies and earths. The following experiments not only confirm this doctrine, but also a position that I have lately ventured to advance*, “that certain decompositions will take place in spirit of wine, which will not at all in water, nor in the *dry way*.”

I have shewn, that if expressed oil be mixed with flaked lime into a paste, so as to form calcareous soap, and mild alkali be added, the latter will not decompose the former, either in water or by fusion. But that if spirit of wine be substituted for water, an alkaline soap and mild calcareous earth will be formed. As sea salt contains the fossil alkali, and as by your table of affinities acids have stronger attraction to metallic calces than to alkalies, I concluded, that if sea salt were added to a metallic soap, a similar double decomposition would take place.

* In an Appendix to the second edition of the “Elements of the Branches of Natural Philosophy connected with Medicine.”

To try this I took some diachylum, which had been bought at Apothecaries-Hall, and added to it sea salt; then covered them to a sufficient height with spirit of wine, and set the bottle over the fire. Soon after they had boiled, the decomposition of the diachylum began to be apparent. When the boiling had continued some time, I removed the vessel from the fire, and after it had stood a few minutes, decanted the clear liquor while hot; then evaporating it, obtained a true alkaline soap. The residuum of course contained a quantity of calx of lead, combined with marine acid.

But much of the diachylum remained either wholly or partly undecomposed: I therefore added more sea salt and spirit of wine, and obtained a further yield of soap. But though much sea salt remained behind, diachylum was still found in the residuum. I found, indeed, that if the ingredients were previously freed from their water, the process succeeded to somewhat better advantage.

From five ounces of diachylum I did not get quite three ounces of soap. This soap was likewise soft, and contained a portion of oil not combined with a sufficient quantity of alkali. The oil, I suppose, had existed in a similar state in the diachylum: and I remarked, that as the spirit evaporated, it gave out the true soap first, the unsaturated oil not till afterwards; so that the latter might easily be obtained separate from the former.

If too much salt was employed, much of it was taken up by the liquid, and communicated to the soap, at least if the ingredients had not been previously deprived of their water. To separate this salt I dissolved the soap in hot water. When the liquor was cold, the soap floated at top, the salt remaining in the water underneath. If too little salt was used, this inconvenience

inconvenience did not happen, or not in so great a degree, though then less soap was of course obtained.

As diachylum, though with a greater proportion of litharge, and boiled longer than that I had from the Hall, still contained oil not sufficiently saturated, I made the metallic soap in another way. To a solution of sugar of lead in water I added a solution of alkaline soap in the same liquid. A double decomposition took place, the oil uniting with the calx of lead, the alkali with the acid of salt. Using this metallic soap instead of the other, I obtained an alkaline soap harder and more perfect than in the preceding process; but still found that part of the oil remained with the calx of lead in the residuum, and adhered so firmly, that repeated quantities of sea salt and spirit of wine did not wholly separate it.

As I have given this process more with a philosophical view than any other, I have been thus particular in my account of it, to shew that however eligible it may appear at first view, it will not answer for making soap for common sale. The alkali indeed is procured much cheaper than from barilla, as the lead may be revived and re-calcined into litharge. But the whole of the oil or fat cannot easily be converted into soap, though in order the better to effect it, I have mixed sand with the diachylum: and as the oil and litharge must, in the large way, be united by boiling, a considerable part of the former will not be sufficiently saturated. Fuel must be used, not only for forming the metallic soap, but likewise for decomposing that soap, and then distilling off the spirit, which will also require additional time and labour. The quantity of spirit of wine lost, though the process (so far as that liquid is concerned) be performed in a still, will alone nearly counterbalance the saving in respect to alkali. And in the process itself there

there is considerable danger, not only of the spirit taking fire from the carelessness of the workmen, but likewise from the frequent explosions that happen during the decomposition of the metallic soap.

As in the experiment with calcareous earth and mild alkali, so in this, I found that the decomposition would not take place when water was used, nor by fusion. In the latter case, I found that the salt was so strongly attracted, that it quitted its water of crystallisation to unite to the metallic soap. If spirit of wine was added to this mass, a double decomposition took place, as already described.

Instead of sea salt, I added to diachylum GLAUBER's salt, freed from its water of crystallisation by heat. I expected that it would have acted on the metallic soap more speedily than the sea salt; but the contrary appeared on trial. On adding a small quantity of sal sodæ, the decomposition went on better, and sufficiently to shew that the ingredients were capable of acting on each other. And I suppose, from your table, that other neutral and earthy salts will have a similar effect, especially if deprived of any superfluous acid by the addition of a little alkali or earth; though I have not made the trials.

Professor BERGMAN has divided his table into two parts; the affinities as they take place in the *moist*, and in the *dry* way. But these experiments shew, that in the *moist* way the affinities take place differently, according as water, or spirit of wine, is used. Perhaps a like difference would be found on using other liquids, each of which would probably afford a different table: for much depends on the attraction which the ingredients themselves have to the liquid employed, as I have endeavoured to shew in the work before referred to; for the liquid is to be considered as one of the ingredients.

I beg leave to add, Sir, that I think I have since hit upon a better method of making soap, and without spirit of wine; but as I have not yet made all the experiments on this subject that I intended, I cannot at present give you an account of them. If they succeed; I will take the liberty to acquaint you with the result.

I am, Sir, with the greatest respect, &c.

J. ELLIOT.

Great Marlborough-Street,
October 31, 1785.

P. S. Since writing the above I have found, that if mild fixed alkali be added to diachylum in hot water, they unite into a gelatinous mass, which is miscible with the water. This may be considered as a kind of *hepar*. If this substance be put into hot spirit of wine, the decomposition already described takes place. If chalk be substituted for alkali, there is a similar result. I have found that nitre is decomposed by diachylum in spirit of wine. I have also found, that if the compound of diachylum and common salt be put into hot spirit of turpentine, the diachylum is dissolved, but the salt remains at the bottom of the vessel.



VII. *An Account of some minute British Shells, either not duly observed, or totally unnoticed by Authors. In a Letter to Sir Joseph Banks, Bart. P. R. S. by the Rev. John Lightfoot, M. A. F. R. S.*

Read January 26, 1786.

DEAR SIR,

AS you were pleased to think a few shells, which I lately submitted to your inspection, might not be unworthy the notice of the Royal Society; encouraged by so respectable an opinion, I shall now beg leave to lay before you some Drawings which I have caused to be made of them, together with such remarks concerning them as may tend, in some degree, to illustrate their natural history.

The first I shall mention is an univalve, coiled up into a spiral form, the cavity of which is divided into three, four, or more distinct chambers or apartments by solid transverse *septa*, which communicate with each other by a *triradiated* aperture.

These characters accord with no genus of shells, hitherto established, so well as the *Nautilus*. It is true, it has not so many chambers as others of that genus, nor are the apertures of the *septa* of a *tubular* form; but as these, according to the laws of method, are to be considered as marks of a *specific* rather than *generic* nature, so I shall not hesitate to refer the shell under consideration to the family of *Nautilus*, at least till

we

we are authorised, by the discovery of many more of a similar structure, to rank it under a new genus.

That I may give a more full and specific description of this singular shell, it must be observed, that its figure is a flattened spiral, umbilicated on one side, convex on the other, but yet slightly depressed in the centre, measuring in diameter about a quarter of an inch; that it is generally coiled up into four volutions, which are convex above, and so nearly plane beneath as to form an acute or carinated margin; and that each of these volutions, on the upper side, has a narrow thread-like border or fillet on the interior edge. The front view of the mouth is obliquely semioval, the upper edge projecting farther than the lower.

The substance of the shell is very brittle and pellucid, and, when alive, of a reddish brown or chestnut colour throughout, except about three or four faint white lines, which appear like rays running from the central umbilicus to different and nearly equidistant parts of the circumference. These white lines are not straight, but shaped each like a short curve, or comma, on the upper side, and are nothing else but the shades of the *septa* in the cavity of the shell.

Such is its *external* appearance. The internal structure is extremely curious; for the whole cavity is divided into three, four, or five chambers or compartments (according to the age of the shell) at nearly equal distances, by transverse *septa* of a hard white brittle semipellucid substance, resembling agate or enamelled glass. Each of these *septa* has a triradiated aperture not unlike the Greek capital upilon, or the Roman Y, inverted, (λ) through which the animal, by means of its soft compressible and extensible nature, easily contrives to extrude

itself, as much as is necessary, when in search of food, or in the act of moving from one place to another.

It may not be amiss here to observe, that the *septa* above-mentioned are totally foreign, both in *use* and *structure*, from what are called *opercula* in other shells: I mean those temporary covers or stoppers, made use of by many testaceous animals to close up the mouths of their shells, and defend them from injury in their quiescent state.

The *opercula*, however various in substance, are always observed to be *single*, *imperforate*, *moveable* at the will of the animal, and constantly placed, as a security, in the *mouth*, never in any *other part* of the cavity of the shell; whereas the *septa*, in the subject now before us, are repeatedly constructed in several parts of the cavity, are all of them *perforated*, intimately connected with the substance of the shell, and consequently *fixed* and *permanent*, as in all the *Nautili*.

And as to the *use* of these *septa*, though I dare not say what might be the real intention of nature in their formation, yet it will be no presumption to affirm, that they could not be designed for the same purpose as *opercula* in other shells; not only because they are placed where they cannot answer the same end, but more especially on account of their open structure, which intirely excludes them from the possibility of affording a proper defence to the enclosed animal.

Should it be said, that they only serve to point out the different *periods* or *stages* of the shell's growth, and are nothing else but the *limits* or *terminations* of the animal's periodical increase, I will not dispute the opinion; it may perhaps be very true; but supposing it to be so, is it not equally probable, that the transverse *septa* in *all* the *Nautili* are nothing else?

But

But I must not conclude my remarks without taking some notice of the *inhabitant* of this singular shell. It appears to be of the *slug kind*, but differs from the common *land* sorts in this respect, that the *Antennæ* are *filiform*, and the eyes not placed upon their *summits* and *retractile*, but fixed upon the *head* near their bases, as is probably the case in all the truly *aquatic* kinds, at least in all such as I have hitherto examined. The animal is of a soft and flexible nature, and grey brown colour, and has a power of extending itself out of the shell through the aperture of the exterior *septum*; at which time it assumes a *triradiated* shape, not very dissimilar from the aperture itself, or like an inverted Y (Λ), the thickest ray of which is the head and body; one of the lines which form the angle is the tail, and the other is a kind of dorsal ligament, which extends from the back of the animal, through one of the rays of the aperture, and through the whole cavity of the shell, and all its *septa*, to the centre, as may be seen by placing the shell between the eye and the light (see fig. 3. Tab. I.).

In the concise LINNÆAN mode of description this shell may be named,

Nautilus (lacustris) testa spirali compressa umbilicata carinata, anfractibus tribus supra convexis contiguis, apertura semiovata, septis triradiato-perforatis.

The Fresh-water Nautilus.

I find no author who has taken any notice of this shell, except Mr. WALKER, who, in his late curious publication on *Minute Shells*, has described it under the name of

Helix lineata dorso convexo umbilicata margine acuto; and has given a figure of it in the same work, Pl. I. fig. 28.

But this ingenious gentleman is free enough to confess, that its *chambered structure* had entirely escaped his notice,

otherwise he would doubtless not have ranked it among the *Helices*.

The place where the shell is to be found, is in deep ditches of clear water, adhering to the roots of *Carices*. It was collected near Upton Church, not-far from Eton, in Buckinghamshire, in the spring season. Mr. WALKER reports it to be found on flags in Hornhill Brooks, in Kent, but very rare.

The figures annexed will explain what I have been describing much better than words.

Fig. 1. (Tab. I.) The shell of its natural size, with the umbilicated side uppermost.

2. The same with the depressed side uppermost; the dark shade in both shewing how far the cavity of the shell is occupied by the dead animal included.

3. The shell magnified with the *depressed* side uppermost, shewing the live animal within it, its head and *antennæ* protruded. Here the white lines appear double, being the shade of the *septa* on both sides of the shell.

5. The same magnified with the *umbilicated* side uppermost, the head and under side of the animal appearing to view.

4. The same magnified in a perpendicular view, with the mouth in front, but cut away down to the first *septum*, in order to shew not only the *carina* or keel of the shell, but more especially the exact appearance of the triradiated *septum* nearest the mouth, and in what manner the animal contrives to extrude itself through the aperture, the head and tail being accommodated to pass through two of the parts of the inverted Y (Λ), while the *dorsal ligament* occupies the third.

8. The animal's excrement.

7. Horizontal sections of the shell, in order to shew the internal structure, or the appearance of the *septa*, when the shell

shell is ground down or divided in that direction. Fig. 6. shewing the shell ground away in part, with its umbilicated side uppermost. Fig. 7. the same more deeply and evenly ground, with the depressed or more convex side uppermost.

The *second* shell I shall take notice of has much of the same external face with the preceding, and is nearly of the same size and colour, but materially differs from it in having an uninterrupted cavity from the mouth to the center; that is, *no divided chambers or compartments*. This therefore evidently belongs to the genus of *Helix*.

It is strongly umbilicated on one side, and almost plane on the other, the central wreaths being nearly of equal height, or but slightly depressed, and destitute of that narrow border or fillet mentioned in the preceding shell. It consists most commonly of three volutions, convex on both sides, with an obtusely carinated margin, and semioval mouth.

It may be named,

Helix (fontana) testa compressa obtusè carinata, hinc umbilicata, anfractibus tribus utrinque convexis, apertura semiovata.

Fountain Helix.

The figures here given represent this shell, on both sides, in its natural and magnified state, so that more words to describe it are needless.

Fig. 1. (Tab. II.) The shell of the natural size, with the most convex side uppermost.

2. The same, with the umbilicated side uppermost.
3. The shell magnified, the most convex side uppermost.
4. The same magnified, the umbilicated side uppermost.

I do not find that it has been noticed by any author.

It

It was found in the bottom of a spring of clear water, adhering to the under side of rotten leaves, near Bullstode, in Buckinghamshire, in the month of April. It has also been found in some other clear waters in the same neighbourhood, but not common.

A *third* shell I have to mention is a very minute but curious *Helix* of a subconical form, consisting of about five convex wreaths, gradually diminishing towards the apex. The shell is umbilicated at the base, and the wreaths are transversely surrounded with numerous sharp-edged rings, which are produced in the middle or back of each wreath into a kind of spur, formed of compressed and very tender spines. The mouth is a segment larger than a semicircle, but not round enough to constitute the shell a *Turbo*, to which it is nevertheless nearly allied. The colour of the whole shell is brown.

It may be named,

Helix (spinulosa) testis subconica umbilicata, anfractibus 5 convexis, annulis membranaceis acutis cinctis, dorso spinuloso-carinatis, apertura suborbiculari.

Tender prickly *Helix*.

The figures here given represent this shell in different positions, in its natural and magnified state.

Fig. 1. 2. (Tab. II.) The shell, in different positions, of the natural size.

3. 4. 5. The same magnified.

I know no author who has hitherto noticed it.

It was found near Bullstode, at the foot of pales, upon old bricks and stones, after rainy weather, in June and July.

A fourth

A *fourth* is a minute shell of the *Turbo* kind.

It strongly resembles the depressed *Helices*; but its circular mouth forbids its being ranked in that *genus*.

It consists of four cylindric or rounded volutions, of nearly equal height on one side, but sunk or umbilicated on the other. These volutions are transversely surrounded with numerous sharp-edged membranaceous rings, which are very fragile and deciduous. The mouth, when perfect, is bordered with a compressed erect margin. The colour of the shell is uniformly brown.

It may be named,

Turbo (helicinus) testa depresso-plana, hinc umbilicata, anfractibus 4 torosis, annulis numerosis acutis membranaceis cinctis.

The fine-ringed *Turbo*.

The figures herewith exhibit both sides of the shell, in its natural and magnified state.

Fig. 1. 2. (Tab. III). The shell, on both sides, of the natural size.

3. 4. The same, on both sides, magnified.

No author, that I know of, has described it.

It was found near Bullstrode, upon bare stones, in the spring season, and at other times in moist weather.

The *fifth* and *last* shell I have to mention, is a small thin oblong compressed *Patella*, of a horn colour, about a quarter of an inch long, and one-tenth of an inch wide, having a pointed vertex nearest to the lower end, turned downwards, and leaning to one side.

It may be called,

Patella

Patella (oblonga) testā integerrima oblonga compressa membranacea, vertice mucronato reflexo obliquo.

Oblong fresh-water *Patella*.

It is perfectly distinct from the *Patella lacustris* of LINNÆUS both in shape, and flexure of the vertex, as well as being destitute of radiated streaks.

Fig. 1. 2. 3. and 4. (Tab. III.) The natural size in different attitudes.

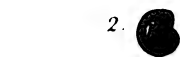
5. A shell magnified, with its vertex upward.

6. *Patella lacustris* LIN. shewing the plan of the two different species.

It has escaped the notice of all the authors I am acquainted with.

It was found adhering to the leaves of the *Iris Pseudacorus* in waters near Beaconsfield, in Buckinghamshire, by Mr. AGNEW, Gardener to the late Duchess Dowager of PORTLAND; by whose sagacity all the preceding shells were discovered, and by whose faithful pencil they were drawn.

I have now done with describing the shells I intended; but before I conclude, it may not be thought, perhaps, quite foreign to my present subject, to remove, in some degree, an error which has been almost universally adopted by the dealers and collectors in shells, respecting certain subjects, brought from Jamaica, and other parts of the West-Indies, commonly known by the name of *Gold Shells*. They are yellow glossy substances, of an obtusely conical figure, and size of tares or vetch-seeds, composed of several concave brittle imbricated scales, closely compacted, so as to resemble the foliaceous gem or bud of some tree, and have generally a hole or perforation in some part. These are commonly supposed to be *shells*, or the
embryos



7.

lus lacustris.



4

fontana.



4

5

spinulosa.

2



4

was.



4



nga.

By fire &c.

embryos of shells taken out of some bag or ovary. It is certain, however, that this is a mistake; for having collected a few of the largest and most opaque of these *supposed shells*, and such as had no perforation to be found in them, I immersed them for a few minutes in hot water, and then carefully developing the scales of which they were composed, I found in the centre of all the largest and most perfect a small insect, enveloped in a mealy substance, about the size of a small bed-bug, of a roundish oval figure, dark brown colour, convex on the back, slightly concave beneath, and in every instance, except *one* (out of at least fifty which I opened), all without wings. The body was composed of about eight imbricated segments or rings; the head was very short, and almost concealed under the margin of the thorax; however, I plainly discerned, in some of the specimens, that it was furnished with two short filiform *antennæ*. The trunk had six legs; the feet terminated each with a sharp red claw. The body of the *single specimen* which had *wings* was oblong, and narrower than the *apterous* ones. The wings appeared to be glued down to the body, just as in a bee or wasp, when it is almost ready to *emerge* from the *Pupa* state. Whether they were two or four wings I am not absolutely certain; but they appeared to be of the filmy transparent kind, at least near the extremities; for I clearly perceived the nerves as in the wings of a fly. From hence it evidently appears, that these *Gold Shells* are really no other than the *cases* or cells of an insect in its *Pupa* state; and from considering the *form of its body*, the *difference of the sexes*, the one being *apterous*, the other *winged*, I have no doubt but it is a species of cochineal or *coccus*, and probably one not hitherto described by naturalists. The *cases* do not effervesce with acids, therefore they are not of a *testaceous* nature. They seem to be a vegetable

table substance of the *resinous* kind ; for they bubble a little on being burnt on a hot iron, and when triturated dissolve slowly in a warm spirituous menstruum to a sweet-smelling viscid matter. But we must wait for a better elucidation of the subject from those who collect these substances in their native place.

I have the honour to be, with the utmost respect, &c.

JOHN LIGHTFOOT.

VIII. *Observations on the Sulphur Wells at Harrogate, made in July and August, 1785. By the Right Reverend Richard Lord Bishop of Landaff, F. R. S.*

Read February 2, 1786.

IN 1733, when Doctor SHORT first published his Treatise on Mineral Waters, there were only three sulphur wells at Harrogate; there are now four. I made some inquiry respecting the time and occasion of making the fourth well, and received the following account from an old man, who was himself principally concerned in the transaction. About forty years ago, a person who, by lease from the Earl of BURLINGTON, had acquired a right of searching for minerals in the forest of Knareborough, made a shew as if he had a real intention of digging for coal, on the very spot where the three sulphur wells were situated. This attempt alarmed the apprehensions of the inn-keepers and others at Harrogate, who were interested in the preservation of the wells: they gave him what legal opposition they could, and all the illegal that they durst. At length, for the sum of one hundred pounds, which they raised amongst themselves, the dispute was compromised, and the design real or pretended of digging for coal was abandoned. Sulphur water, however, had risen up where he had begun to dig. They inclosed the place with a little stone edifice, and putting down a bason, made a fourth well. By a clause in the act of parliament for inclosing Knareborough Forest, passed

in 1770, it is rendered unlawful for any person whatever to sink any pit, or dig any quarry or mine, whereby the medicinal springs or waters at Harrogate may be damaged or polluted; so that no attempts of the kind above-mentioned need be apprehended in future.

This fourth well is that which is nearest to one of the barns of the Crown-Inn, being about ten yards distant from it. In digging, a few years since, the foundation of that barn, they met with sulphur water in several places. At a very little distance from the four wells there are two others of the same kind; one in the yard of the Half-Moon-Inn, discovered in digging for common water in 1783, and another which breaks out on the side of the rivulet below that Inn. On the banks of that rivulet I saw several other sulphureous springs: they are easily distinguished by the blackness of the earth over which they flow.

On the declivity of a hill, about a quarter of a mile to the west of the sulphur wells at Harrogate, there is a bog which has been formed by the rotting of wood: the earth of the rotten wood is in some places four feet in thickness, and there is a stratum consisting of clay, and small loose decaying sand-stones, every where under it. The hill above is of grit-stone. In this bog there are four more sulphur wells; one at the top, near the rails which separate the bog from the Common; and three at the bottom, though one of these, strictly speaking, is not in the bog but at the side of it in the stratum on which the bog is situated, and at the distance of a yard or two from a rivulet of fresh water, which runs from thence to Low Harrogate, passing close to the side but above the level of the sulphur wells of that place. On the other side of the hill, above the bog, and to the west of it, there is another sulphur well on the side of a brook; and it has been thought that the wells both at Harrogate and in the bog

bog, are supplied from this well. In a low ground; between High Harrogate and Knaresborough, there is a sulphur well; another to the north of it in Bilton Park, at about the distance of a mile; and another to the south of it, at a less distance, was discovered this year in digging for common water by a person of the name of RICHARDSON; and, lastly, there is another at a place called Hookstone Crag: none of these last mentioned wells are above two miles distant from High Harrogate; and by an accurate search a great many more might, probably, be discovered in the neighbourhood.

It is not unusual to dig within a few yards of any of these sulphur wells, and to meet with water which is not sulphureous. I ordered a well to be dug in the fore-mentioned bog, sixteen yards to the south of the sulphur well which is near the rails, and to the same depth with it; the water with which it was presently filled was chalybeate, but in no degree sulphureous. I had another well dug, at about thirty yards distance from the three sulphur wells which are situated at the lower extremity of the bog; this well, by the declivity of the ground, was ten or twelve feet below their level, but its water was not sulphureous. From the first well which I dug, it is evident, that every part of the bog does not yield sulphur water; and from the second, which was sunk into the clay, it is clear that every part of the stratum on which the bog is placed does not yield it, though one of the wells is situated in it.

The sulphur wells at Harrogate are a great many feet below the level of those in the bog; but they communicate with them, if we may rely on what Doctor SHORT has told us — “That about the beginning of this century, when the concourse of people was very great to the Spaw at Harrogate, one ROBERT WARD, an old man, made a basin in the clay under the
the

the moss of a bog where the strongest and briskest of these sulphur springs rise, and gathered half an hoghead of water at a time for the use of the poor; but when he laded this he almost dried the three sulphur wells at the village, whence it is evident, that all have the same origin and communicate with one another." By conversing with some of the oldest and most intelligent people at Harrogate, I could not find that they entertained any opinion of the water at the bog having a communication with that at the spaw. This circumstance might easily be ascertained; and, if the fact should be contrary to what Doctor SHORT supposed, the wells at the bog ought to be covered from the weather as those at the village are; they would by this mean yield great plenty of water for the baths which are wanted by invalids, and which are often very scantily supplied by the wells at Harrogate, notwithstanding the attention which is used in preserving the water which springs at the four wells, by emptying them as often as they become full during both the day and night time. And indeed it is surprising, that the well on the side of the rivulet below the Half-Moon-Inn, which is so well situated for the purpose, has never been inclosed for the furnishing sulphureous water for the baths. The present mode of carrying the water in casks to the several houses where the persons lodge who want to bathe in it, is very troublesome, and the water thereby loses of its virtue. Some of the wells about the village, that for instance which has been discovered at the Half-Moon-Inn, the water of which, I believe, springs from a different source from that which supplies the four sulphur wells, should be either enlarged to a greater horizontal breadth, or sunk to a greater depth, in order to try, by one or both of these ways, whether the quantity and strength of the water might not be increased;

and if that should, as it probably would be the case, one or more baths might be erected after the manner of those at Buxton and other places; or, by proper additional buildings, warm bathing in sulphureous water might be practised, as is done in common water in the bagnios in London. The saltiness of the sulphureous water, if that should be thought useful, might easily be made even greater than that of sea water, by adding a quarter of a pound of common salt to every gallon of the water used in forming a bath. The waters at Harrogate, though they have long been very beneficial, have not yet been rendered so useful to mankind, as an intelligent and enterprising person might make them. The alternate strata of sand, stone, and shale, which compose the lower hills near the wells at Harrogate, dip very much, as may be seen in a stone quarry about two hundred yards from the wells; and the same circumstance may be observed in dry weather, in following the bottom of the brook from the village up to the bog; and hence, if there be a communication between the waters of the bog and of the village, as Doctor SHORT asserts, it is probable, that the same stratum of shale which is seen at the bottom of the wells at the village, breaks out again at the bog above the village, and that the water finds its way from the bog to the village through the crevices of that stratum.

After having observed, as carefully as I could, the number and situation of the sulphur wells about Harrogate, I took notice of the temperature of the four at the village. In the month of June, 1780, when the thermometer in the shade was 72° , and the pump water at the Granby-Inn, the well of which is fifty feet deep, was 48° , the strongest of the sulphur wells, being that of which invalids usually drink, was 50° . On the 29th of July in this year, after the earth had been parched with

with drought for many months, the heat of the strongest well was 54° ; the water of the Granby pump was on the same day 48° , and the heat of the air in the shade 76° . Doctor WALKER, who has lately written a treatise on Harrogate water, says, that the heat of this spring was 48° , when that of an adjoining rivulet was 53° . And I have little doubt in believing, that if the experiment was made in cold weather, the temperature of the same well would be found to be several degrees below 48 . This variation of temperature in the sulphur water indicates its springing from no great depth below the surface of the earth; or at least it indicates its having run for a considerable distance in a channel so near to the surface of the earth, as to participate of the changes of temperature, to which that is liable from the action of the sun. But the heat of the sulphur water is not only variable in the same well, at different times, but it is not the same in all the wells at the same time. If we call the strongest well the first, and reckon the rest in order, going to the right, the third well, which is reckoned the next strongest, was 57° hot when the first well was 54° . In support of the conjecture that the sulphur water of the strongest well would in a cold season make the thermometer sink below 48° , which is the constant temperature of springs situated at a great depth in the earth in this country, it may be observed, that though the first and the third well are never frozen, yet the second and the fourth well are frozen in severe weather. When the second and the fourth well are covered with ice, it is probable, that the first and the third have a temperature far below 48° ; but that the sea salt, which is more abundant in them than in the other two wells, and which of all salts resists most powerfully the congelation of the water in which

it is dissolved, preserves them from being frozen in the coldest seasons incident to our climate.

As the temperature of these four wells is not the same in all of them at the same time, nor invariable in any of them, so neither does there seem to be any uniformity or constancy in them, with respect to the quantity of salt which they contain. The salt with which they are all impregnated is of the same kind in all, and it is almost wholly common salt; and though the quantity contained in a definite portion of any one of the wells is not, I think, precisely the same at all seasons of the year, yet the limits within which it varies are not, I apprehend, very great. A method is mentioned in the LXth volume of the Philosophical Transactions, of estimating the quantity of common salt dissolved in water, by taking the specific gravity of the water: this method is not to be relied on, when any considerable portion of any other kind of salt is dissolved along with the sea salt; but it is accurate enough to give a good notion of the quantity contained in the different wells at Harrogate. On the 13th of August, after several days of rainy weather, I took the specific gravities of the four sulphur wells at the village, the drinking well being the first.—Rain water 1.000; first well 1.009; second well 1.002; third well 1.007; fourth well 1.002. By comparing these specific gravities with the table which is given in the LXth volume of the Transactions, it may be gathered, that the water of the first well contained $\frac{1}{75}$ of its weight of common salt; that of the second and fourth, $\frac{1}{135}$; and that of the third, $\frac{1}{14}$. After four days more heavy rain I tried the strongest well again, and found its specific gravity to be 1.008. It is worthy of observation, that the water, as it springs into the first and third well, is quite transparent, but usually of a pearl colour in the second and

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fourth, similar in appearance to the water of the first or third well after it has been exposed a few hours to the air; hence it is probable, that the external air has access to the water of the second and fourth well before it springs up into the basin. A great many authors have published accounts of the quantity of common salt contained in a gallon of the water of the strongest well; they differ somewhat from each other, some making it more, others less, than two ounces. These diversities proceed either from the different care and skill used in conducting the experiment; or from a real difference in the quantity of salt with which the water is impregnated at different seasons of the year. The medium quantity of salt contained in a gallon falls short of, I think, rather than exceeds two ounces. The sea water at Scarborough contains about twice as much salt as is found in the strongest sulphur well at Harrogate. The sulphur wells at the bog are commonly said to be sulphureous, but not saline. This, however, is a mistake; they contain salt, and salt of the same kind as the wells at the village. I could not distinguish the kind of salt by the method in which I had estimated the quantity contained in the sulphur wells; I therefore evaporated a gallon of the water of the well in the bog which is near the rails, and obtained a full ounce of common salt, of a brownish colour: the colour would have gone off by calcination. In what degree the medicinal powers of Harrogate water depend on its sulphureous, and in what degree on its saline impregnation, are questions which I meddle not with: I would only just observe on this head, that any strong sulphureous water, such as that of Kettlestone in Derbyshire, or of Shap in Westmoreland, which naturally contains little or no sea salt, may be rendered similar to Harrogate water, by dissolving in it a proper proportion of common salt. The four
sulphur

Sulphur wells at Harrogate are very near to each other; they might all be included within the circumference of a circle of seven or eight yards in diameter; yet, from what has been said it is evident, that they have not all either the same temperature, or the same quantity of saline impregnation. This diversity of quality, in wells which have a proximity of situation, is no uncommon phenomenon; and though at the first view it seems to be surprising, yet it ceases to be so on reflexion: for the waters which feed wells so circumstanced, may flow through strata of different qualities situated at different depths, though in the same direction; or through strata placed both at different depths, and in different directions; and that this is the case at Harrogate is probable enough, there being hills on every side of the hollow in which the village is placed.

With respect to the sulphureous impregnation of these waters, I made the following observations.

The inside of the basin, into which the water of the strongest well rises, is covered with a whitish pellicle, which may be easily scraped off from the grit-stone of which the basin is made. I observed, in the year 1780, that this pellicle on a hot iron burned with the flame and smell of sulphur. I this year repeated the experiment with the same success; the substance should be gently dried before it is put on the iron. I would further observe, that the sulphur is but a small part of the substance which is scraped off. That I might be certain of the possibility of obtaining true palpable sulphur from what is scraped off from the basin, and at the same time give some guess at the quantity of sulphur contained in it, I took three or four ounces of it, and having washed it well, and dried it thoroughly by a gentle heat, I put two ounces into a clean glass retort, and sublimed from it about two or three grains of

A a 2

yellow

yellow sulphur. This sulphur, which stuck to the neck of the retort, had an oily appearance; and the retort, when opened, had not only the smell of the volatile sulphureous acid, which usually accompanies the sublimation of sulphur, but it had also the strong empyreumatic smell which peculiarly appertains to burnt oils; and it retained this smell for several days. It has been remarked before, that the salt separable from the sulphur water was of a brownish colour; and others, who have analysed this water, have met with a brown substance, which they knew not what to make of; both which appearances may be attributed to the oil, the existence of which was rendered so manifest by the sublimation here mentioned. I will not trouble the Society with any conjectures concerning the origin of this oil, or the medium of its combination with water; the discovery of it gave me some pleasure, as it seemed to add a degree of probability to what I had said concerning the nature of the air with which, in one of my Chemical Essays, I had supposed Harrogate water to be impregnated. I will again take the liberty of repeating the query which I there proposed. "Does this air, and the inflammable air separable from some metallic substances, consist of *oleaginous* particles in an elastic state?" When I ventured to conjecture, in the Essay alluded to, that sulphureous waters received their impregnation from air of a particular kind, I did not know that Professor BERGMAN had advanced the same opinion, and denominated that species of air, Hepatic Air. I have since then seen his works, and very readily give up to him not only the priority of the discovery, but the merit of prosecuting it. And though what he has said concerning the manner of precipitating sulphur from these waters can leave no doubt in the mind of any chemist concerning the actual existence of sulphur

in them; yet I will proceed to the mention of some other obvious experiments on the Harrogate water, in support of the same doctrine.

Knowing that, in the baths of Aix-la-Chapelle, sulphur is found sticking to the sides and top of the channel in which the sulphureous water is conveyed, I examined with great attention the sides of the little stone building which is raised over the basin of the strongest well, and saw them in some places of a yellowish colour: this I thought proceeded from a species of yellow moss, commonly found on grit-stone: I collected, however, what I could of it by brushing the sides of the building, at the distance of three or four feet from the water in the basin: on putting what I had brushed off on a hot iron, I found that it consisted principally of particles of grit-stone, evidently however mixed with particles of sulphur.

Much of the sulphureous water is used for baths at Harrogate; and for that purpose all the four wells are frequently emptied into large tubs containing many gallons apiece; these constantly stand at the wells, and the casks, in which the water is carried to the several houses, are filled from them. On examining the insides of these tubs, I found them covered, as if painted, with a whitish pellicle. I scraped off a part of this pellicle: it was no longer soluble in water; but, being put on a hot iron, it appeared to consist almost wholly of sulphur. Some of these tubs have been in use many years, and the adhering crust is thick in proportion to the time they have been applied to the purpose; but the sulphur pellicle was sufficiently observable on one which was new in the beginning of this season. The water when it is first put into these tubs is transparent; when it has been exposed to the air for a few hours, it becomes milky; and, where the quantity is large, a white cloud:

cloud may be seen slowly precipitating itself to the bottom. This white precipitate consists partly, I am not certain that it consists wholly, of sulphur; and the sulphur is as really contained in the waters denominated sulphureous, as iron is contained in certain sorts of chalybeate waters; in the one case the iron is rendered soluble in water by its being united to fixed air, or some other volatile principle; and in the other sulphur is rendered soluble in water by its being united to fixed air, or some other volatile principle: neither, iron nor sulphur are of themselves soluble in water, but each of them, being reduced into the form of a salt by an union with some other substances, becomes soluble in water, and remains dissolved in it, till that other substance either escapes into the air, or becomes combined with some other body.

About forty years ago, they took up the basin of the third well, and a credible person, who was himself present at the operation, informed me, that in all the crevices of the stone on which the basin rested, there were layers of pure yellow sulphur. This I can well believe, for I ordered a piece of shale to be broken off from the bottom of the fourth well; it was split, as shale generally is, into several thin pieces, and was covered with a whitish crust. Being laid on a hot iron, in a dark room, it cracked very much, and exhibited a blue flame and sulphureous smell.

If the water happens to stand a few days in any of the wells, without being disturbed, there is found at the bottom a black sediment; this black sediment also marks the course of the water which flows from the well, and it may be esteemed characteristic of a sulphur water. The surface of the water also, when it is not stirred for some time, is covered with a whitish scum. Doctor SHORT had long ago observed, that
both

both the black sediment, and the white scum, gave clear indications, on a hot iron, of their containing sulphur: I know not whence it has come that his accuracy has been questioned in this point; certain I am, that on the repetition of his experiments I found them true. The white scum also, which is found sticking on the grafs over which the water flows, being gently dried, burns with the flame and smell of sulphur. From what has been said it is clear, that sulphur is found at Harrogate, sticking to the bason into which the water springs, sublimed upon the stones which compose the edifice surrounding the well; adhering to the sides of the tubs in which the water stands; subsiding to the bottom of the channel in which the water runs; and covering the surface of the earth, and of the blades of grafs, over which it flows. It is unnecessary to add another word on this subject; it remains that I risk a conjecture or two, on the primary cause of the sulphureous impregnation observable in these waters.

In the Chemical Essay before referred to, I have shewn, that the air separable from the lead ore of Derbyshire, or from Black-Jack, by solution in the acid of vitriol, impregnates common water with the sulphureous smell of Harrogate water; and I have also shewn that the bladder fucus or sea-wrack, by being calcined to a certain point, and put into water, not only gives the water a brackish taste, but communicates to it, without injuring its transparency, the smell, taste, and other properties of Harrogate water. Professor BERGMAN impregnated water with a sulphureous taste and smell, by means of air separated by the vitriolic acid from *hepar sulphuris*, made by fusion of equal weights of sulphur and pot-ashes, and from a mass made of three parts of iron filings melted with two of sulphur; and he found also, that Black-Jack and native Siberian

iron yielded hepatic air, by solution in acids. This, I believe, is the main of what is known by chemists on this subject; what I have to suggest, relative to the Harrogate waters in particular, may perhaps be of use to future inquirers.

I have been told, that on breaking into an old coal-work, in which a considerable quantity of wood had been left rotting for a long time, there issued out a great quantity of water smelling like Harrogate water, and leaving, as that water does, a white scum on the earth over which it passed. On opening a well of common water, in which there was found a log of rotten wood, an observant physician assured me, that he had perceived a strong and distinct smell of Harrogate water. Dr. DARWIN, in his ingenious Account of an artificial Spring of Water, published in the first part of the LXXVth volume of the Philosophical Transactions, mentions his having perceived a slight sulphureous smell and taste in the water of a well which had been sunk in a black, loose, moist earth, which appeared to have been very lately a morass, but which is now covered with houses built upon piles. In the bog or morass above-mentioned there is great plenty of sulphureous water, which seems to spring from the earth of the rotten wood of which that bog consists. These facts are not sufficient to make us certain, that rotten wood is efficacious in impregnating water with a sulphureous smell; because there are many bogs in every part of the world, in which no sulphureous water has ever been discovered. Nor, on the other hand, are they to be rejected as of no use in the inquiry; because wood, at a particular period of its putrefaction, or when situated at a particular depth, or when incumbent on a soil of a particular kind, may give an impregnation to water, which the same wood, under different circumstances, would not give.

The

The bilge water, usually found at the bottom of ships which are foul, is said to smell like Harrogate water: I at first supposed, that it had acquired this smell in consequence of becoming putrid in contact with the timber on which it rested, and this circumstance I considered as a notable support to the conjecture I had formed of rotten wood being, under certain circumstances, instrumental in generating the smell of Harrogate water. But this notion is not well founded; for the bilge water is, I suppose, salt water; and Dr. SHORT says, that sea water, which had been kept in a stone bottle six weeks “stunk not much short of Harrogate sulphur water.” It has been remarked above, that calcined sea-wrack, which contains a great deal of sea salt, exhales an odour similar in all respects to that of Harrogate water; and in confirmation of the truth of this remark, I find that an author, quoted by Dr. SHORT, says, that “Bay salt thrice calcined, dissolved in water, gives exactly the odour of the sulphur Well at Harrogate.” From these experiments considered together, it may, perhaps, be inferred, that common salt communicates a sulphureous smell to water both by putrefaction and calcination. Hence some may think, that there is some probability in the supposition, that either a calcined stratum of common salt, or a putrescent salt spring, may contribute to the production of the sulphureous smell of Harrogate water; especially as these waters are largely impregnated with common salt. However, as neither the salt in sea water, nor that of calcined sea-wrack, nor calcined bay salt, are any of them absolutely free from the admixture of bodies containing the vitriolic acid, a doubt still remains, whether the sulphureous exhalation, here spoken of, can be generated from substances in which the vitriolic acid does not exist.

The shale from which alum is made, when it is first dug out of the earth, gives no impregnation to water; but by exposure to air and moisture its principles are loosened, it shivers into pieces, and finally moulders into a kind of clay, which has an aluminous taste. Alum is an earthy salt resulting from an union of the acid of sulphur with pure clay; and hence we are sure, that shale, when decomposed by the air, contains the acid of sulphur; and from its oily black appearance, and especially from its being inflammable, we are equally certain that it contains phlogiston, the other constituent part of sulphur. And indeed pyritous substances, or combinations of sulphur and iron, enter into the composition of many, probably of all sorts of shale, though the particles of the pyrites may not be large enough to be seen in some of them; and if this be admitted, then we need be at no loss to account for the bits of sulphur, which are sublimed to the top of the heaps of shale, when they calcine large quantities of it for the purpose of making alum: nor need we have any difficulty in admitting, that a phlogistic vapour must be discharged from shale, when it is decomposed by the air. Dr. SHORT says, that he burned a piece of aluminous shale for half an hour in an open fire; he then powdered and infused it in common water, and the water sent forth a most intolerable sulphureous smell, the very same with Harrogate water. He burned several other pieces of shale, but none of them stunk so strong as the first. This difference may be attributed, either to the different qualities of the different pieces of shale which he tried, or to the calcination of the first being pushed to a certain definite degree; for the combination of the principles on which the smell depends may be produced by one degree of heat, and destroyed by another. I have mentioned, briefly,

briefly, these properties of shale, because there is a stratum of shale extended over all the country in the neighbourhood of Harrogate; several beds of it may be seen in the stone quarry above the sulphur wells; many of the brooks about Harrogate run upon shale, and the sulphur wells spring out of it. They have bored to the depth of twenty yards into this shale, in different places, in search of coal, but have never penetrated through it. Its hardness is not the same at all depths. Some of it will strike fire, as a pyrites does, with steel; and other beds of it are soft, as if in a state of decomposition, and the sulphur water is thought to rise out of that shale which is in the softest state. But whatever impregnation shale when calcined, or otherwise decomposed to a particular degree, may give to the water which passes over it; it must not be concluded, that shale in general gives water a sulphureous impregnation; since there are many springs, in various parts of England, arising out of shale, in which no such impregnation is observed.

I forgot to mention, in its proper place, that having visited the bog, so often spoken of, after a long series of very dry weather, I found its surface, where there was no grass, quite candied over with a yellowish crust, of tolerable consistency, which had a strong aluminous taste, and the smell of honey. BERGMAN speaks of a turf found at Helsingberg in Scania, consisting of the roots of vegetables, which was often covered with a pyritous cuticle, which, when elixated, yielded alum; and I make no doubt, that the Harrogate morass is of the same kind.

Whether nature uses any of the methods which I have mentioned of producing the air by which sulphureous waters are impregnated, may be much questioned; it is of use, however, to record the experiments by which her productions may be

imitated; for though the line of human understanding will never fathom the depths of divine wisdom, displayed in the formation of this little globe which we inhabit; yet the impulse of attempting an investigation of the works of God is irresistible; and every physical truth which we discover, every little approach which we make towards a comprehension of the mode of his operation, gives to a mind of any piety the most pure and sublime satisfaction.

IX. *Observations and Remarks on those Stars which the Astronomers of the last Century suspected to be changeable.* By Edward Pigott, Esq.; communicated by Sir Henry C. Englefield, Bart. F. R. S. and A. S.

Read February 9, 1786.

IT is about a century since HEVELIUS, MONTANARI, FLAMSTEED, MARALDI, and CASSINI, noticed a certain number of stars which they supposed had either disappeared, changed in brightness, or were new ones; and yet to this day we have acquired no further knowledge of them. This may be attributed to the difficulty of finding out what star is meant, and the not having exact observations of their relative brightness. I therefore have drawn up the following catalogue, and made the necessary observations; so that in future we can examine them without much trouble, and be certain of any change that may take place. To accomplish this, it was requisite to compare with attention many authors and most of the catalogues of stars; in doing which I have perceived several undoubted errors, and others highly probable; but as entering into a discussion of such disagreements would swell this account considerably, and make it very intricate, I shall avoid, as much as possible, any thing of the kind that is not immediately necessary.

In order to separate certainty from doubt, I have classed these stars in two divisions; the first are undoubtedly changeable;
the

the others remain yet to be better authenticated. Though some of them bear all the appearance of being variable, still no *certainty* of their being so has come to my knowledge. To those of the first class are subjoined observations made on them within these last four years, from which the period and progressive changes of some are deduced, though never settled before, and if already known are more exactly determined by comparing my observations with former ones. Also, as the position of several are determined only by ancient astronomers, and therefore inaccurately, I have observed them with great exactness, the declinations being taken with a BIRD's eighteen-inch quadrant, and the right ascensions with a three-foot transit instrument: these last may serve in future to discover their proper motions in right ascension, for which reason I shall specify the stars to which they were compared. The stars of the second class have either their relative brightness exactly settled, or their non-existence ascertained. I have also pointed out the probability of a mistake in several, and in general given an account of the appearance they have had within these few years.

Catalogue of variable Stars, reduced to the beginning of 1786.

Class the first.

Names.	R. A. in time.			Declination.			Greatest and least magnitudes.	From whence reduced.
	h.	'	"	°	'	"		
Nova 1572, in Cassiopea,	00	13	00	62	58	+ N	1 — 0	{ RICCIOLUS's Almagestum, &c.
• Ceti	2	8	33	3	57	25 S	2 — 0	BRADLEY.
Algol	2	54	19	40	6	55 N	2 — 4	MAYER.
MAYER's 420th in Leo	9	36	5	12	25	00 N	6 — 0	MAYER.
In Hydra	13	18	4+	22	9	38 S	4 — 0	From my observat.
Nova 1604 in Serpentarius	17	18	00	21	20	½ S	1 — 0	Phil. Tr. N° 346.
• Lyrae	18	42	11	33	7	46 N	3 — 4.5	BRADLEY.
Near the Swan's head	19	38	58	26	48	½ N	3 — 0	Phil. Tr. N° 65.
• Antinoi	19	41	34	00	28	14 N	3.4 — 5	LA CAILLE.
In the Swan's neck	19	42	21	32	22	58 N	5 — 0	From my observat.
In the Swan's breast	20	9	54	37	22	37 N	3 — 0	From my observat.
• Cephei	22	21	00	57	20	00 N	4.3 — 4.5	FLAMSTEED.

Class the second.

HEVELIUS's 6 Cassiopeæ	00	23	16	60	50	00 N	7 — 0	HEVELIUS.
46 or ½ Andromedæ	1	9	46	44	24	00 N	4.5 — 5.6	FLAMSTEED.
50 or • Andromedæ	1	24	16	40	20	15 N	4.5 — 0	FLAMSTEED.
HEVELIUS's 41 Androm.	1	28	40	41	31	½ N	5 — .	HEVELIUS.
TYCHO's 20th Ceti	1	39	.	13	20	. S	5 — 0	TYCHO.
55 or Neb. Andromedæ	1	40	30	39	40	3 N	6 — .	FLAMSTEED.
PROL. and UL. BEIGH } • Eridani	2	42	.	9	40	. S	4 — 0	UL. BEIGH.
41 Tauri	3	53	27	27	00	39 N	5 — 0	FLAMSTEED.
47 Eridani	4	23	54	8	41	40 S	4 — 0	FLAMSTEED.
Near 53d Eridani . . .	4	29	00	12	30	± S	4 — 0	By estimation.
• Canis Majoris.	6	54	5	15	19	36 S	3 — 0	LA CAILLE.
• Geminorum	7	32	11+	28	31	38 N	1 — 3	MASKELYNE.
½ Leonis	9	20	24	12	14	23 N	4 — 6	MAYER.
¼ Leonis	9	32	3	14	59	36 N	5.6 — 0	MAYER.
25th Leonis	9	46	8	12	20	36 N	6.7 — 0	FLAMSTEED.
BAYER's ½ Leonis . . .	9	52	½	15	30	. N	6 — 0	TYCHO.
• Ursæ Majoris	12	4	45	58	13	24 N	2 — 4	LA CAILLE.

Class the second continued.

Names.	R. A. in Time.	Declination.	Greatest and least magnitudes.	From whence reduced.
	h. ' "	° ' " N		
α Virginis	12 7 43	00 24 16 N	6 0	MAYER.
BAYER's * near γ η	12 53 00	10 00 . S	6-0	From maps.
In N. thigh of Virgo	13 29 +	00 30 . S	6-0	From maps.
ρ Virginis	13 43 43	2 5 50 N	6-0	FLAMSTEED.
α Draconis	13 58 36	65 24 8 N	2-4	BRADLEY.
In west scales of Libra	14 53 $\frac{1}{2}$	13 26 . S	6-0	{ Mém. de l'Acad. des Sciences.
PTOL. and UL. BEIGH's } 6th unformed in Libra }	15 29 +	20 30 . S	4-7	UL. BEIGH.
α Libræ	15 29 39	19 58 27 S	4- .	LA CAILLE.
TYCHO's 11th Libræ	15 37 $\frac{1}{2}$	19 30 . S	4-0	TYCHO.
33 Serpentis	15 38 00	17 14 00 N	6-0	FLAMSTEED.
Near α Ursæ Minoris	16 $\frac{1}{4}$.	82 $\frac{1}{4}$. N	6-0	From maps.
PTOL. 14 Ophiuchi	17 2 14	26 15 37 S	4-0	BRADLEY.
PTOL. 13 Ophiuchi	17 18 +	20 35 . S	4-0	PTOL.
PTOL. 18 Ophiuchi	17 22 .	24 10 . S	5-0	PTOL.
α Sagittarii	18 42 00	26 32 34 S	2-4	MAYER.
θ Serpentis	18 45 35	3 56 26 N	4-5	LA CAILLE.
TYCHO's 27th Capricor.	21 41 .	14 28 . S	6-0	TYCHO.
TYCHO's 22d Androm.	21 43 $\frac{1}{2}$	49 15 . N	4-0	TYCHO.
TYCHO's 19 Aquarii	22 25 .	15 55 . S	6-0	TYCHO.
α Andromedæ . . .	22 52 6	41 10 45 N	4-6	LA CAILLE.
LA CAILLE's 483 } Zodi. Cat. . . .	22 55 40	8 50 45 S	7-0	LA CAILLE.

I shall now proceed to give a short account of these stars, and begin with those of the first class.

The famous Nova of 1572 in Cassiopea.

Several astronomers are of opinion, that it has a periodical return, which KEILL and others have conjectured to happen every 150 years. This is also my opinion; and I cannot think its not being noticed at the completion of every term a material

rial objection, since perhaps, as with most of the variables, it may at different periods have different degrees of lustre, so as sometimes to increase only to the ninth magnitude; and if this be the case, its period is probably much shorter. This induced me, in September 1782, to take a plan of the smallest stars near its place, and which I have examined often since, but found no alteration.

o Ceti.

Since the end of 1782 I have observed very exactly the decrease of brightness of this star; but never have seen it of above the 6th magnitude. Oct. 29, 1782, it was of the 7th magnitude, and gradually decreased till Dec. 30, it being then of the 8 . 9th magnitude.

1783, Feb. 16, certainly less than the 9th magnitude.

1783, August 25, of the 6th magnitude, and gradually decreased until Dec. 14, being then of the 10th magnitude, and equal to the little star close to it.

1784, Jan. 11, I thought it by intervals still less than the same little star.

1784, Sept. 12, it was of the 7 . 8th magnitude, and gradually decreased until Dec. 9, and then was of the 9th magnitude, and rather brighter than the little star.

As a matter of curiosity, I have deduced its period from the times when it was equal to a certain star in the course of its decrease; the results were 320—337 and 328 days; but M. CASSINI determined its mean period with greater exactness to be 334 days. Mr. GOODRICKE saw it Aug. 9, 1782, of the 2d magnitude, rather brighter than α and less than β Ceti. Sept. 5, it was of the 3d magnitude, being equal to γ Ceti.

Algol.

The period of Algol, discovered by Mr. GOODRICKE, gave us some new light into the nature of the fixed stars. Though the phænomena seem to attract the attention of most astronomers, still there are some points which require further investigation. Its degree of brightness, when at its *minimum*, is different in different periods; and also, I think, when at its full, it is sometimes brighter than α Persei, and at other times less. Whether these differences return regularly after a certain number of periods remains yet to be examined. My last observations, when it was at the middle of its *minimum*, are,

- h.
- 1785, July 8, at 11 50 undoubtedly less than ϵ Persei.
 — July 31, at 9 50 equal to ϵ Persei.
 — Sept. 12, at 10 45 { a remarkable observation; rather less than δ Persei;
 { evidently brighter than ϵ ; nearly of the 3d mag.

MAYER's N° 420, lately discovered to be variable by M. KOCH.

A few years before 1782, M. KOCH saw the N° 420 undoubtedly less than the N° 419 of MAYER's Catalogue.

In February 1782, he found them both exactly of the same brightness, therefore of the 7th magnitude.

From an extract of a letter I have lately seen, the variable was of the 9th magnitude in April, 1783, and of the 10th in April, 1784.

I have often seen the N° 419, but never the variable, though I have frequently looked for it with a night-glass, and on the 4th of April, 1785, in a 3-foot achromatic transit instrument.

Variable

Variable in Hydra.

MARALDI, in 1704, having found that this star had a periodical variation, continued to examine it for several years, and concluded its period to be about two years, though with considerable variations; in which he was much mistaken, as will appear from the following results, which shew that its period in all probability is tolerably regular, and only of 494 days.

Dates when it was at the middle of its greatest brightness,
estimated from MARALDI's observations.

1704, March 14, he saw it nearly of the same magnitude
from the beginning of March until the
beginning of April; it then decreased.

1705, . . . he saw it very faint in November, 1705,
and found it decreasing: this observation
is too imperfect.

1708, May 22, accurately determined; its increase and decrease
being well observed.

1709, Nov. 10, :: doubtful, its decrease only being observed.

1712, May 1, :: ditto, ditto, ditto.

1784, Jan. 26, by me, very accurately, its increase and
decrease being observed. See the Observations that conclude this paper.

1785, May 27, ditto, ditto, ditto.

The four greatest intervals of MARALDI's Observations give
for single periods in days thus. 495—517—480 and 510, the
mean being $500\frac{1}{4}$, which is tolerably exact considering how

doubtful the observations of 1709 and 1712 are. My two make it 487 days; but as the interval is only a single period, it may err 10 days; I therefore shall take a mean between the results, which is 494, and proceed on to the following comparisons of MARALDI's two best observations with mine.

1708, May 22,	}	interval of 56 periods, each of $493\frac{2}{3}$ days.
1784, Jan. 26,		
1708, May 22,	}	interval of 57 periods, each of $493\frac{1}{2}$ days.
1785, May 27,		
1704, Mar. 14,	}	interval of 59 periods, each of $494\frac{1}{2}$ days.
1784, Jan. 26,		
1704, Mar. 14,	}	interval of 60 periods, each of $494\frac{1}{3}$ days.
1785, May 27,		

A single period, on a mean, 494 days.

If MARALDI's observations of 1704 and 1708 are exact to a month, and there is no reason to believe otherwise, the period at that time seems to have been a few days longer than it is at present, and therefore the one here deduced may be esteemed as the mean period.

Particulars of the changes it undergoes.

1. When at its full brightness it is of the 4th magnitude, and has no perceptible change for about a fortnight.
2. It is about six months in increasing from the 10th magnitude, and returning to the same.
3. Therefore it may be considered as invisible also during six months.
4. It is considerably quicker in increasing than in decreasing, perhaps by half.

Though

Though when at its full it may always be stiled of the 4th magnitude, it does not constantly attain exactly the same degree of brightness, but the differences are very small, as shewn below.

1704, brighter than ψ .

1708, brighter than ψ .

1784, { much brighter than ψ , being nearly
between ψ and γ Hydræ.

1785, rather brighter than ψ .

Its mean right ascension, computed from my observations, and reduced to Jan. 1, 1784, is

199 29 30 { from 4 observations, compared to ζ π , made between March
and May 1784.

199 29 21 from 2 ditto, compared to MAYER's 538, made in May, 1784.

199 29 20 from 5 ditto, compared to γ Hydræ, made between March and May, 1784.

199 29 24 — mean right ascension for Jan. 1, 1784, on a mean.

HEVELIUS's 30th Hydræ is the above star; he marks it of the 6th magnitude; I find it in no other Catalogue.

The famous Nova of 1604, in Serpentarius.

A full account of this star is given by KEPLER, and it seems to have had a similar appearance to the Nova in Cassiopea; therefore the reflections delivered there need not be again repeated. In July, 1782, I took a plan of the smallest stars near its place, which was examined every year since, but no alteration was perceived.

β Lyræ.

Mr. GOODRICKE discovered the variation and period of this star, and hopes soon to settle its different phases with more exactness;

exactness; I shall therefore not enter into any detail, being certain it cannot be in better hands. In his last account he mentions having first suspected the period to be only of six days nine hours; such has always been my opinion, and which material point will probably be more satisfactorily determined in his next publication.

Nova near the Swan's Head of 1670.

This star was first seen in December 1669 by Don ANTHELME; it soon became of the 3d magnitude, and disappeared in 1672, after having undergone several variations. I have constantly looked for it since November, 1781, without success; had it increased to only the 10th or 11th magnitude, I should have perceived it, having taken an exact plan of all the surrounding stars.

η Antinoi.

The variation and period of this star I discovered last year, and had the honour of communicating an account of it to the Society: as at present a long interval is elapsed since my first observations, and that lately I have noted some of its phases with exactness, I shall compare them to those observed in 1784, which of course will give results more satisfactory. The period, as settled in my former paper, is 7 d. 4 h. 38'; but for reasons there alledged, it must be much less precise than the following.

1785, July 18, at 9 h.	} times when η Antinoi was between its least and greatest brightness.
Sept. 6, at 18	
Sept. 27, at 22	

These being compared to similar observations of Sept. 12 and 19, 1784, make the length of a single period thus :

D.	H.	M.
7	4	12
7	4	19
7	4	17
7	4	22
7	4	7
7	4	12
<hr/>		
7	4	15 on a mean.

I see no reason to alter materially the other points; but believe them more exact thus :

40 hours at its greatest brightness.

66 — in decreasing.

30 — at its least.

36 — in increasing.

It also, in every period, seems to attain the same degree of brightness when at its full, and to be equally decreased.

Variable in the Swan's Neck.

During these three years I have observed this star with particular attention, as may be seen by the observations that conclude this Paper, and determined the middle time of its greatest brightness very exactly, thus :

1783, July 9, of the 6 . 7th magnitude.

1784, Aug. 4, of the 5 . 6th —

1785, Sept. 1, of the 6th —

The

The second of these, being compared to that of Nov. 20, 1687, made by KIRCH, gives 406 days exactly for one period, the interval between them being 35322 days, and divided by 87 periods. I make the divisor 87, in order to get a result nearest to that settled by MARALDI and CASSINI of 405, and by M. LE GENTIL of 405,3 days. We cannot suppose that these great astronomers have made any mistake; and on the other hand, it seems hardly possible, that the mean of my observations alone, which makes the period 392 days, can err 14; but perhaps its period is irregular; to determine which several intervals of 15 years ought to be taken, and I am much inclined to believe, that it will be found of only 396 days 21 hours.

Particulars of the changes it undergoes.

1. When at its full brightness it has no perceptible change for about a fortnight.
2. It is about $3\frac{1}{2}$ months in increasing from the 11th magnitude to its full brightness, and the same in decreasing.
3. Therefore it may be considered as invisible during six months.
4. It does not attain the same degree of brightness at every period, being sometimes of the 5th, and other times of the 7th magnitude.

Its mean right ascension, computed from my observations, and reduced to Aug. 1, 1783, is

295 33 46 from 2 observ. compared to γ Cygni, made in July and August, 1783.

295 33 45 from 2 ditto, compared to γ Lyræ, ditto.

295 33 45 from 1 ditto, compared to α Lyræ, made in August, 1783.

295 33 55 from 3 ditto, compared to β Lyræ, made in July and August, 1783.

295 33 52 from 2 ditto, compared to β Cygni, made in August 1783.

295 33 48½ mean right ascension for August 1, 1783, on a mean.

Variable in the Swan's Breast.

This star was first seen by G. JANSONIUS in 1600, and afterwards frequently observed by different astronomers, but with intervals of ten or more years, which is probably the reason why no regularity in its changes has yet been deduced. I have examined minutely the observations made in the last century, and shall venture to give the following results.

1. Continues at its full brightness for about five years.
2. Decreases rapidly during two years.
3. Invisible to the naked eye for four years.
4. Increases slowly during seven years.
5. All these changes, or its period, are completed in 18 years.

6. It was at its *minimum* at the end of the year 1663.

It does not always increase to the same degree of brightness, being sometimes of the 3d, and at other times only of the 6th magnitude. I am intirely ignorant whether it is subject to the same changes since this century, having not met with any series of observations on it; but if the above conjectures are exact, it will be at its *minimum* in a very few years. Since November, 1781, I have constantly seen it of the 6th magnitude, being rather less than N° 28, 29, and *m*, and rather brighter than

36 and 40 Cygni. Sometimes I suspect it has rather decreased within these two last years, though in a very small degree.

Its mean right ascension, computed from my observations, and reduced to Sept. 1, 1782, is

302 26 43 from 3 observations, compared to γ Cygni, made in October, 1781.

302 26 46 from 3 ditto, compared to γ Cygni, made in August, 1782.

302 26 52 from 1 ditto, compared to β Aquilæ, made ditto.

302 26 46 from 1 ditto, compared to α Cygni, made ditto.

302 26 39 from 1 ditto, compared to δ Andromedæ, made in October 1781.

302 26 45 mean right ascension for Sept. 1, 1782, on a mean.

FLAMSTEED has this star in his Catalogue; but, I believe, observed it only once.

δ Cephei.

This is the last variable star discovered, and again by Mr. GOODRICKE. Its changes are very difficult to be seen, unless examined when at its *minimum* and full brightness. I have lately made some good observations on it thus:

1785, Aug. 30, at 14 h. less than ϵ Cephei.

31, at 9 h. equal if not brighter than ζ Cephei.

Sept. 15, at 12½ h. less than ϵ Cephei.

16, at 8 h. between ϵ and ζ Cephei.

— at 11 h. increased, but not as bright as ζ .

17, at 11 h. rather brighter than ζ .

26, at 11½ h. equal or less than ϵ .

27, at 8 h. evidently brighter than ζ .

Therefore it was between its least and greatest brightness August 31, at noon, and Sept. 26, at 21 h.: these being compared to my first observations, when also between its least and greatest brightness on Nov. 20, at 3 h. and Nov. 30, at 15 h.

1784,

1784, give the following results, the mean of which corroborates that deduced by Mr. GOODRICKE of 5 d. 8 h. 37 $\frac{1}{4}$.

D. H. M.

5 8 35

5 8 41

5 8 33

5 8 39

Length of a single period 5 8 37 on a mean.

3 Cephei concludes the stars of the first class; those that follow are of the second.

HEVELIUS's 6th Cassiopeæ.

In 1782 I first perceived that this star was missing; nor could I find it in 1783 and 1784.

46th or ξ Andromedæ.

This star is said to have diminished in brightness. In 1784 and 1785 I found it, by very exact observations, less than ν , equal to ω^* , and brighter than d and χ ; yet I must mention that it is marked in my journal as being sometimes brighter, and at other times less than ω^* ; but still I am not convinced, that it varies in brightness. FLAMSTEED, in his Catalogue, annexes no character to his 46th Andromedæ; but in vol. II. of his Hist. Cœlest. p. 135. and 138. he marks it ξ .

* I suspect an error in this character, but cannot be certain. H. E.

FLAMSTEED'S 50, 52, τ Andromedæ, and HEVELIUS'S 41 Andromedæ.

As the position and characters of these stars differ considerably in different Catalogues, and that some of them are mentioned by CASSINI to have disappeared and re-appeared, I shall give their brightness as observed in 1783, 1784, and 1785.

FLAMSTEED'S 50th of the 4. 5th magnitude, and equal, if not rather less than ϕ Andromedæ.

———— τ of the 5th magnitude, and equal to 46 and 48 Andromedæ.

————	49	} of the 5. 6th magnitude, and are of the same brightness.
————	52	
HEVELIUS'S	41	

A star between FLAMSTEED'S 52 and HEVELIUS'S 41 is of the 6th magnitude, or rather less. I could not see TYCHO'S 19th Andromedæ; but I take this star to be the same as HEVELIUS'S 41 Andromedæ.

TYCHO'S 20th Ceti.

This must be the star which HEVELIUS said had disappeared, being TYCHO'S second in the Whale's belly. There can hardly be any doubt but that it is the χ , misplaced by TYCHO. This χ is of the 4. 5th magnitude, and of the same brightness as the three ψ Aquarii.

FLAMSTEED'S 55th Andromedæ, marked Neb. in his Catalogue.

It is mentioned in the latest Catalogues of Nebulæ that this nebula could not be found. FLAMSTEED, who, I believe,

only observed it once, *viz.* Oct. 17, 1691, does not mark it nebulous; nor does it appear to me such, but as a star of the 6th magnitude. There are a few small stars near it, which to the naked eye, when the air is very clear, make it appear nebulous, which probably is the reason why FLAMSTEED marked it thus in his Catalogue.

σ or PTOL. and UL. BEIGH's 17th Eridani.

FLAMSTEED says, he could not see this star in 1691 and 1692. In 1782, 1783, and 1784, I observed one of the 7th magnitude in that place; the relative brightness of which appeared always the same, *viz.* less than two little stars near and below γ Eridani.

FLAMSTEED's 41 Tauri.

This star was thought by CASSINI to be a new one or variable. I see little or no reason to be of that opinion; that it is not new is evident, since it is UL. BEIGH's 26th and TYCHO's 43d. In 1784 and 1785 I found it of the 5th magnitude, being equal to φ, and brighter than ψ, P, and χ Tauri.

Star about 2° ¼ North of 53d Eridani, and 47 Eridani.

The first of these stars CASSINI thought a new one, and that it was not visible in 1664. In 1784 I found it was less than ω, and δ, brighter than A, and seemed equal to ψ Eridani.

CASSINI mentions another star thereabouts, which he also esteemed a new one: this is probably FLAMSTEED's 47th. In 1784 it appeared rather less than 46th.

γ Canis

γ Canis Majoris.

MARALDI could not see this star in 1670; but in 1692 and 1693 it appeared of the fourth magnitude. I have very frequently noticed it since 1782, but perceived not the least variation, being constantly of the 4th magnitude, very little brighter than θ , and decidedly brighter than ι .

 α β Geminorum.

If either of these stars have changed in brightness, it is probably the β . In 1783, 1784, 1785, the β was undoubtedly brighter than α .

 ξ Leonis.

MONTANARI says, this star was hardly visible in 1693. I found it constantly in 1783, 1784, and 1785, of the same brightness, being of the 5th magnitude; less than A, π , and, if any difference, rather brighter than b and ω Leonis. TYCHO, FLAMSTEED, MAYER, BRADLEY, &c. mark it of the 4th magnitude.

 ψ Leonis.

This star is said to have disappeared before the year 1667. It is now, and has ever been since 1783, of the 5 . 6th magnitude, being less than ω , and brighter than i , FLAMSTEED's 46th.

25th Leonis.

In 1783 I first perceived this star was missing; nor was it visible in 1784 and 1785, even with the transit-instrument.

BAYER's ι Leonis, or TYCHO's 16 Leonis.

It was not visible in 1709, nor could I see it in 1785. This is a different star from the ι Leonis of the other Catalogues, though TYCHO's *description* of its place is the same.

 δ Ursæ Majoris.

This star is suspected to change in brightness (see LONG's Astronomy), on account of its being marked by TYCHO, PRINCE OF HESSE, &c. of the 2d magnitude; while HEVELIUS, BRADLEY, and others, have it of the 3d. At present, and for these three years past, it appears as a bright 4th magnitude, being rather less than ι , equal to α , and rather brighter than κ Draconis.

 η Virginis.

This star is supposed to be variable, because FLAMSTEED, on the 27th of January, 1680, says he could not see it. He observed it May 12, 1677, and some years afterwards, since it is in his Catalogue. I examined it frequently in 1784 and 1785, without perceiving the least change, being of the 6th magnitude, less than c , and *rather* brighter than a star three degrees lower in a right line with c and η Virginis.

BAYER's.

BAYER's star of 6th magnitude, 1° South of γ Virginis.

This star is not in any of the nine Catalogues that I have. MARALDI looked for it in vain; and in May, 1785, I could not see the least appearance of it. It certainly was not of the 8th magnitude.

In the northern thigh of Virgo.

This star, which is marked by RICCIOLUS of the 6th magnitude, could not be seen by MARALDI in 1709; nor was it of the 9th magnitude, if at all visible, in 1785.

γ 1 or γ 2. Virginis.

In 1785 I found that one of these stars was missing, and which seems to be the γ 1; the remaining one is of the 6.7th magnitude.

α Draconis.

I am of Mr. HERSCHEL's opinion, that it is highly probable this star is variable. BRADLEY, FLAMSTEED, &c. mark it of the 2d magnitude; at present it is only of a bright 4th. I have frequently examined it since October, 1782, without perceiving the least change, being constantly rather less than α Draconis, equal to δ Ursæ Majoris, and rather brighter than α Draconis.

BAYER's star in the west scales of Libra.

MARALDI says he could not see this star; nor could I in 1784 and 1785. With a night-glass may be seen thereabouts
some

some small stars of about the 8th magnitude, none of which are near as bright as the 2d γ Libræ.

PTOL. and UL. BEIGH'S N° 6 of the unformed in Libra.

In examining different Catalogues I do not find this star in any other than the above, though it is marked of the 4th magnitude. If PROLEMY had not the κ it might be thought to be that. In 1785 I frequently observed a star of the 7th magnitude very near its place, which appeared rather less than FLAMSTEED'S 41. FLAMSTEED has not this little star in his Catalogue; but he observed it May 9, 1681.

κ Libræ.

This star is thought to be variable. I am not of that opinion; though certainly it is rather singular that HEVELIUS, whose attention was directed to this part of the heavens, to find TYCHO'S 11th, did not observe the κ ; and the more so, as he has noticed two much lesser stars not far from it. During these three years I have found the κ constantly of the 5th magnitude, being less than ψ or θ , equal to λ , and brighter than η .

TYCHO'S 11th Libræ.

HEVELIUS says he could not find a star of the 4th magnitude in Libra noticed by TYCHO. This must be TYCHO'S 11th, since he has all the others. It was not visible in 1783, 1784, and 1785, nor probably ever existed; for it is, I think, evident, that this 11th is no other than the κ , with an error of two degrees in longitude.

33 Serpentis.

In 1784 I perceived that this star was missing; nor was it visible in 1785 with a night-glass.

A star marked by BAYER near ϵ Ursæ Minoris.

CASSINI could not see this star. In 1782 I took, with a night-glass, a plan of all the stars near its place, and near the ϵ , none of which were brighter than the 7.8th magnitude. I have since re-examined the plan, but found no alteration.

The ρ or PTOL. and UL. BEIGH's 14th Ophiuchi or FLAMSTEED's 36th.

I have no doubt but that this is the star which is said to have disappeared before 1695. It is also evident, by what HEVELIUS says in his Catalogue on the θ and B, that the ρ was not seen by him. In 1784 and 1785 I found it of the 4.5th magnitude, much brighter than 39, also rather brighter than 51 and 58, and less than 44. On the 30th of June, 1783, I have marked it in my journal equal to 39, and less than 51 and 58; but as the observation was not repeated, I am far from being certain it has undergone any change, particularly as this star has a southern declination of 26° , and therefore great attention must be given to the state of the atmosphere.

PTOL.

PTOL. 13th and 18th Ophiuchi, 4th magnitude.

If there is no error in the Catalogue, these two stars have disappeared; but I am confident that PTOLEMY's 13th is FLAMSTEED's 40th, and that PTOLEMY's 18th ought to be marked with a north latitude instead of south, which would make it agree nearly with FLAMSTEED's 58th.

σ Sagittarii.

MR. HERSCHEL, with great reason, has placed this star among those which probably have changed their magnitudes. I had long since remarked the singular disagreement in all the Catalogues, which induced me to observe it frequently, particularly in 1783, 1784, and 1785, when it appeared of the 2.3d magnitude, and brighter than π Sagittarii.

θ Serpentis.

MONTANARI says he saw this star of the 5th magnitude, and that the next year it grew bigger. I examined it frequently in 1783, 1784, and 1785, and found it always less than δ Aquilæ, equal to β Aquilæ, and P Ophiuchi; 4th magnitude.

TYCHO's 27th Capricorni.

This star was not visible in HEVELIUS's time; nor could I see it 1778, 1782, 1784, with the transit-instrument.

E c 2

TYCHO's

TYCHO's 22d Andromedæ and σ Andromedæ.

CASSINI remarked, that the star placed by TYCHO at the end of the chain of Andromeda as of the 4th magnitude, was grown so small that it could scarcely be seen. This is TYCHO's N^o 22, the longitude and latitude of which places it near the two π Cygni, and where no star was visible in 1784 and 1785.

As possibly, by TYCHO's description, CASSINI took the 22d for the σ Andromedæ, I have also examined this star, and in 1783, 1784, and 1785, found its relative brightness thus: less than α Cephei; equal to ζ Cassiopeæ, though, if any difference, rather brighter; and brighter than λ , μ , or ι Andromedæ.

TYCHO's 19th Aquarii.

This is the star that HEVELIUS says was missing, and that FLAMSTEED could not see with his naked eye Nov. 18, 1679; nor could I see the least appearance of it in 1782. I am convinced it is the same star as FLAMSTEED's 56th, marked *f* by BAYER, from which it is only $1^{\circ}\frac{1}{4}$. FLAMSTEED's 53d, marked *f* in PTOLEMY's Catalogue, is a different star.

LA CAILLE's 483 Aquarii.

I first discovered that this star was missing in 1778. It was not visible in 1783, 1784.

There are a few other stars suspected by the ancient astronomers to have been new or altered a little in brightness, which I have omitted, not seeing any reason to think them so; and some that are certainly variable, but cannot be observed in these

these latitudes. I have also, contrary to my first intention, added several which are not mentioned by them; such are those that I lately discovered to be missing.

Perhaps many persons would place in the first class several stars which I have put in the second, relying on the positive assertions we have of their having disappeared, diminished, or being new; for my part, I am confident that most of these supposed changes may be attributed to mistakes; and in general for those that are said to be lost, an attentive comparison of different Maps and Catalogues will point out the error; and thus I have ventured to give my opinion of TYCHO's 20th Ceti, 11th Libræ, and 19th Aquarii, &c. Since FLAMSTEED's Catalogue has been more particularly investigated, the number of these supposed lost stars is considerably increased; but if the second volume of his *Hist. Cœlest.* is also examined, many errors will be detected; among which it will appear very unaccountable, that the 71st, 80th, and 81st of Hercules, which were discovered to be missing by Mr. HERSCHEL, are not in FLAMSTEED's observations under the name of Hercules, though I looked for them with particular attention, and find the 70th. The 19th Persei, which Mr. HERSCHEL also could not see, was observed but once by FLAMSTEED, *viz.* Jan. 16, 1693, and in all probability is the τ with the time of its transit erroneously set down. The τ was observed on Jan. 17, 1693, and Jan. 18, 1694. Besides these Mr. GOODRICKE has found several other errors still more evident. I scarcely need add, that these corrections do not in the least intimate any mistake or diminish the merit of those that first point them out, but fall entirely on the ancient catalogues and observations.

Mr. HERSCHEL, in selecting several stars which possibly may be reckoned new ones, very judiciously gives us plausible reasons
not

not to lay great stress on their being so; and the following remark only confirms what he there suggests; for that star of the 5th magnitude following τ Persei, mentioned by him, and which with great reason might be esteemed a new one, is in all probability the same as one observed by FLAMSTEED, Jan. 18, 1694, though not inserted in his Catalogue.

With regard to those stars which are said to have diminished or increased, as those in Andromeda, Leo, &c. they are, in my opinion, far from being confirmed as variable. I know, from repeated experience, that even more than a single observation, if not particularised and compared with neighbouring stars, is very little to be depended on; different states of the weather, thin streaks of clouds, have several times made me err a whole magnitude in the brightness of a star.

Whether these apparent changes in the stars proceed intirely from themselves; or whether they are effected by any foreign power that may in part occasion some of their particular appearances and irregularities, we have not sufficient *data* to determine: but whatever are the causes, a division of the different phænomena seems to be the most probable means of forwarding any conjecture that hereafter may be formed; I shall therefore divide the first class into three orders.

The first contains those that are periodical with long intervals; and such I reckon α Ceti, that in Hydra, that in the Swan's breast and neck, and also MAYER's N° 420.

For the second order I shall mention only three, though others might be added; but the accounts of them are so unsatisfactorily recorded, and their places so little known, that I prefer selecting only that in Cassiopea of 1572, that in Serpentarius of 1604, and that near the Swan's head. The phænomena of these certainly bear a great resemblance to the first:

first: still their sudden appearance, and no certainty of a period, or at least infinitely longer, are, I think, sufficient reasons to separate them.

Lastly, Algol, η Antinoi, β Lyræ, and δ Cephei, are so similar to each other, and so different from the above, that there can be but little or no hesitation in distinguishing them; also the cause of their changes seem in general to be attributed to spots, and a rotation on their axis. This property of the fixed stars, though often suspected, was far from being evident till within these two years; and we are not only indebted to Mr. GOODRICKE for the discovery of the first, but also for three of the only four known.

Further may be added, that all those of the first order (MAYER's 420 being yet so little known remains doubtful) attain in different periods different degrees of brightness when at their full; also the progressive increase of brightness of that in Hydra, that in the Swan's breast, of α Ceti, β Lyræ, η Antinoi, and δ Cephei, is not similar to their decrease. This peculiarity with regard to Algol is yet uncertain, owing to the rapidness of its changes, so that there is only one that seems to have these points uniform, viz the variable in the Swan's neck.

I shall now conclude with the observations from which some results, given in this Paper, have been deduced; they are here collected together, in order to avoid confusion.

Observations-

Observations on the variable in Hydra.

* Variable π	NORTH γ	* * k ψ	γ of 3d magn.
	SOUTH		ψ of 4th — k of 6.7th — π of 8th — π of 11th —

- 1783, Dec. 11. A.M. much less than ψ , and rather less than k .
 24. A.M. equal, if not less than, ψ ; of the same colour as ψ .
- 1784, Jan. 4. A.M. { brighter than ψ ; of about $\frac{1}{4}$ of the difference between
 ψ and γ ; of a more copper colour than ψ .
 9. A.M. } if any difference brighter.
 10. A.M. }
25. A.M. { with the naked eye, it appeared in brightness nearly between
 ψ and γ ; and of a more copper colour than either ψ or γ .
 Feb. 1. } of the same brightness, but seemed of a more copper
 2. } colour.
 23. between ψ and k ; air not clear.
 29. } between ψ and k , but nearer the brightness of ψ .
- March 10. }
- April 11. }
 12. } rather brighter than k .
 19. }
- May 2. }
9. rather less than k , and brighter than π .
- 1784, Dec. 1. A.M. did not see the variable; strong moon-light.
 9. A.M. ditto ditto.
 22. A.M. ditto ditto.
- 1785, Mar. 4. or 11. ditto ditto.
- April 17. saw the π , but not the variable.
- May 4. { visible to the naked eye; less than ψ , and much brighter
 than k ; of a more copper colour than ψ .
 7. } rather less than ψ .
 11. }

May

- 1785, May 14. equal to \downarrow ; air not very clear, and moon-light.
 15. equal to \downarrow ; air clear; moon-light.
 19. undoubtedly brighter than \downarrow ; little hazy; moon near.
 22. } rather brighter than \downarrow ; moon-light strong.
 27. }
- June 10. rather brighter than \downarrow , though I think decreased.
 12. equal to \downarrow : but am not sure if the sky was quite clear.
 13. rather less bright than \downarrow ; air clear.

Mr. GOODRICKE also frequently observed the variable, and his observations agree with the above.

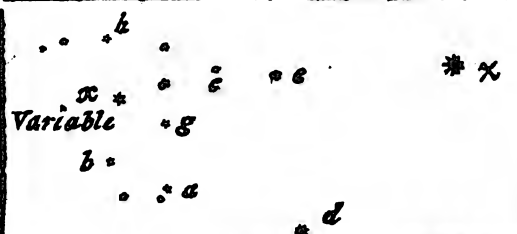
Observations on γ Antinoi.

h.

- 1785, May 20. at $12\frac{1}{2}$ equal or less than γ Antinoi.
 21. — $12\frac{1}{2}$ equal to γ , evidently less than β ; moon-light.
 22. — $12\frac{1}{2}$ equal if not brighter than β Aquilæ; moon.
 — — $12\frac{1}{2}$ thought it brighter than β ; air clear.
- June 10. — $12\frac{1}{2}$ rather brighter than γ Antinoi.
 12. — $11\frac{1}{2}$ rather brighter than γ , less than β .
 13. — $12\frac{1}{2}$ a little brighter than β , much less than δ .
- July 15. — $12\frac{1}{2}$ less than β , brighter than γ .
 17. — 11 less than γ , brighter than μ .
 18. — 11 between γ and β , I think rather nearer β .
 19. — $10\frac{1}{2}$ } between β and δ , rather nearer β ; air clear.
 $11\frac{1}{2}$ }
20. — 11 rather brighter than β , certainly equal.
- Aug. 30. — $9\frac{1}{2}$ rather brighter than β , at least equal to it.
 31. — 9 much brighter than β .
- Sept. 6. — 9 less than γ ; a single view of it, not very satisfactory.
 7. — $9\frac{1}{2}$ much brighter than β .
 26. — $9\frac{1}{2}$ } much less than β or γ , brighter than μ .
 $10\frac{1}{2}$ }
27. — 8 } much less than β , less than γ , brighter than μ .
 $10\frac{1}{2}$ }
28. — $9\frac{1}{2}$ rather brighter than β .
 — — 11 increased a little in brightness.

The brightness of the stars to which η Antinoi is compared are given in the Philosophical Transactions, vol. LXXV. part I.

Observations on the variable in Cygnus's Neck.

	x of 6th mag.	As KIRCH's plan (see Phil. Trans. abridged by JONES) is much the same as this, I have annexed the same letters to the stars.
	d of 7th —	
	e of 7th —	
	a of 8th —	
	b of 8th —	
	c of 8.9 —	
	b of 9th —	

Dates.	Mag.	
1783		
Mar. 28	0	if visible, not of the 8th magnitude.
June 9	7	rather brighter than d or e .
11	7	rather increased.
23	6.7	less than x .
30	6.7	rather increased.
July 15	6.7	of the same brightness as on the 30th of June.
27	7	a little brighter than d or e .
Aug. 7	7	rather brighter than d ; decreased.
8	7	equal to d .
16	7	less than d , brighter than e .
25	7.8	less than e .
Sept. 4	7.8	think brighter than a .
12	8	equal to a .
22	8.9	much less than a , equal to e , and brighter than b .
24	9	equal to b .
Oct. 14	9.10	less than b .
24	10.11	{ seen with the greatest difficulty; less than any stars of the plan; am doubtful if I saw it by intervals.
27		
1784	0	{ I looked constantly for the variable between October and April, but could not see it.
April 23	10.11	less than any stars of the plan.
24	10.11	ditto.
May 9	9.10	rather less than b .

Dates.	Mag.	
1784		
May 21	8 . 9	rather brighter than <i>b</i> , but not so bright as <i>b</i> .
June 17	8	equal to <i>a</i> , brighter than <i>b</i> .
July 21	6	equal to χ with the naked eye.
22	6	if any difference the χ was the brightest.
27	6	rather brighter than χ .
Aug. 1	5 . 6	undoubtedly brighter than χ .
10	6	think it decreased, but still rather brighter than χ .
13 }	6	rather less than χ .
19 }		
Sept. 2	6 . 7	much decreased, less than χ , but brighter than <i>d</i> .
12	6 . 7	still brighter than <i>d</i> .
19	7	if any difference brighter than <i>d</i> .
20	7	rather less than <i>d</i> .
Oct. 5	7 . 8	less than <i>e</i> , about equal to <i>a</i> .
16	7 . 8	rather brighter than <i>a</i> .
Nov. 11	9	less than <i>b</i> , about equal to <i>f</i> .
17	10	rather less than <i>f</i> .
1785		
May 9	0	not visible.
June 21	9 . 10	less than <i>b</i> .
July 23	8	rather brighter than <i>a</i> .
Aug. 13 }	7	{ a little brighter than <i>d</i> and <i>e</i> , much less than χ ; am not sure
15 }		{ I could see it with the naked eye.
27 }	6 . 7	{ not so bright as χ , much brighter than <i>d</i> ; I could see it
28 }		{ with the naked eye, but the χ more distinctly.
30	6	not quite so bright as χ ; naked eye.
Sept. 2 }	6	ditto
7 }		ditto.
11 }	6 . 7	{ very difficult to see with the naked eye, decreased in
12 }		{ brightness; air clear.
26 }	7	{ though the air was remarkably clear, it was with the utmost
27 }		{ difficulty I could sometimes see it with the naked eye.



X. *An Account of a Subsidence of the Ground near Folkestone; on the Coast of Kent. In a Letter from the Rev. John Lyon, M. A. to Edward King, Esq. F. R. S. and A. S. Communicated by Mr. King in a Letter to Charles Blagden, M. D., Sec. R. S.; with Remarks.*

Read February 16, 1786.

TO DR. BLAGDEN, Sec. R. S.

DEAR SIR,

Mansfield-Street, Dec. 22, 1785.

HAVING always thought the account given in the Philosophical Transactions, by Mr. SACKETTE*, about the beginning of this century, concerning the motion of the Cliffs, and of the adjacent ground, near Folkestone in Kent, a very curious one, and deserving of much attention; both because of the many positive attestations there were of *ancient* men with regard to it (who were both mariners, and used to observation); and because of the singular consequences that would follow, from the ascertaining of such a fact, in a philosophical light; I have constantly, whenever I had any opportunity, made repeated enquiries concerning the matter, of such persons as I thought likely to be able to afford me any satisfactory information.

* See Phil. Transf. vol. XXIX. N^o 349. or JENNA'S Abridgment, vol. IV. part II. p. 248.

Amongst

Amongst the rest, I mentioned it last summer to my worthy and very curious friend Mr. BOYS, of Sandwich; who seemed surpris'd at the narration, and had never before heard of any such phænomenon. However, in less than a fortnight after our conversation, I was agreeably surpris'd by receiving a letter (dated 24th Sept. 1785), in which he said, *I am sorry your health will not permit you to make the tour you at first propos'd; especially as something VERY CURIOUS has happened within these few days at Folkestone. Part of the cliff, to the westward of the town, a little way from the church, has sunk, and continues sinking into the earth; raising the ground, about the sinking part, in a very extraordinary manner. This corresponds with what you said to me on the subject of Sturfsall-Castle, &c.; and certainly deserves your attention. If I could, by going thither, give you any satisfaction, I shall be ready and happy to obey your commands.*

Being, through illness, prevented from examining this curious phænomenon myself, I accepted this obliging offer of Mr. BOYS, and requested his assistance: and although he also was prevented from going to the spot himself, yet he applied to a friend of his, the reverend Mr. LYON, of Dover; who has made repeated visits to the place, to obtain all the information possible; and has, at last, sent to me a very accurate drawing, together with an explanatory letter; which I now, with great pleasure, venture to lay before the Royal Society.

I must, however, at the same time, beg leave to observe, that although Mr. LYON differs from Mr. SACKETT, in his conclusion, concerning the motion of the whole adjacent country, and controverts *that* fact; and has certainly given a more clear and satisfactory account of the present phænomenon, than Mr. SACKETT did of that which he wish'd to record; yet they both agree in imputing a *most remarkable effect* (only in different

different ways) to the passage of the springs, and drains of water, through the stratum of loose marle, on which the whole country rests: an effect which must needs produce, at different periods of time, various alterations on the surface; and may most probably have occasioned much greater changes in the face of the country than that made in the present instance.

Whether therefore Mr. SACKETTE was right or wrong, in his great and final conclusion, concerning the motion of the *whole coast*; what he records, on the testimony of so many aged persons (in which they persisted with great seriousness and on the fullest consideration), does surely still deserve at least to be born in mind, and to be attended to with much circumspection; especially on a coast, where perhaps *fixed points* may be attempted to be ascertained, at some time or other, in order to complete the most accurate and most curious philosophical mensurations.

And I must further venture to observe, in vindication of Mr. SACKETTE's account and conclusions, that although Tarlingham-house has indeed been rebuilt, since the time referred to by the old man who conversed with Mr. LYON; and therefore *that* old man's remark might be occasioned merely by *that* circumstance; yet it had not been rebuilt in Mr. SACKETTE's time; and, therefore, no such circumstance could be the occasion of its coming *recently in view*, at that period when he wrote, in parts of the coast where it had not been possible to see it at some time before.

And as to the MOORING ROCK, *so particularly referred to by Mr. SACKETTE, being now utterly unknown*; it ought to be remembered, that Mr. SACKETTE, in his description of it, says, *that it lies surrounded with great numbers of other rocks, and was on this account chiefly a noted one, because at it vessels use to*

be moored, while they are loading other rocks, which they take from hence, not only for our own pier-heads, but for those of Dover Pier; and a very great quantity of them were shipped in the time of OLIVER's usurpation, and carried to Dunkirk, for the service of that harbour. Considering, therefore, that the enormous pile of Ramsgate Pier has been built since that time, (which, though it be chiefly composed of Portland stone, had, I apprehend, foundations and interior parts of ruder materials) and that there have been other vast demands for stone, it is not at all unlikely, that this very *Mooring Rock*, mentioned by Mr. SACKETTE, has itself been carried away in like manner as the others were that used to surround it; and that this is the sole reason why it is now no longer known, and totally forgotten.

I am, Sir, with much esteem, &c.

EDWARD KING.

P. S. There is a *peculiarity* in Mr. LYON's sketch, Tab. V. (designed to illustrate the grounds of his objections to Mr. SACKETTE's conclusions) which demands some explanation. After having given a section of the cliff and shore, the lines (instead of being continued in the same plane, and in the direction of the *same* section) are drawn so, as to be conceived as extended *on the surface of the country* from the eye of the observer at E. Without attending to this circumstance, what he says is not very easily to be understood; and indeed I must still think, that Mr. SACKETTE does not deserve so much censure, although Mr. LYON's be undoubtedly a most accurate account, and most clear solution of the present phenomenon.

TO

TO EDWARD KING, ESQ.

S I R,

Dover, Nov. 24, 1785.

AS I have been requested by my friend Mr. Boys, of Sandwich, to examine into the cause of the sinking of some ground near the town of Folkestone, in this neighbourhood, and to send you the results of my inquiries; I have made it my business to attend particularly to the subject. I have been twice to view the place. I have endeavoured to procure the best information, and have compared my remarks with what the reverend Mr. SACKETT formerly said upon the same subject to the Royal Society.

That you may have a clearer idea of the place where the ground is sinking, I have annexed a drawing of it, taken from a small hill near the foot of the cliff.

AA (Tab. IV.) represents the length of the ground, 130 feet, which is sunk 40 feet from the top of the cliff DD.

BB, is a fissure, in the valley between the sinking ground AA and the hill HH, and in which there are many smaller chasms.

C, the tower of Folkestone-Church, not far from the cliff DD.

E, part of the town of Folkestone, as seen between the cliff DD and the hill HH.

F, the high chalk cliffs at a distance, leading towards Dover.

G, a track of pasture land, between a high range of hills and the sea.

I, the beach, at the foot of the hill H.

KK, Rocks, said to be raised (and I believe they are) by the sinking of the ground AA.

As I intend, in explaining the cause of the sinking of the ground AA to you, to advance an opinion of my own, and to controvert what the reverend Mr. SACKETT formerly said upon the subject, it may be necessary to explain the nature of the soil, as far as it is open to view, in the neighbourhood of Folkestone.

The chalk cliffs FF, which begin at Dover, form opposite Folkestone town high hills, and leaving the shore, there is a large track of arable and pasture land between them and the sea.

Part of this ground is shewn in the view at G, and is a kind of marle, which contains pyrites, fragments of the Cornu Ammonis, and many other fossil bodies.

Next to the marle is a loose sandy soil (see the cliff DD) intermixed with a very large, hard, and coarse kind of stone, in which are often found fossil oyster shells.

This sandy soil rests upon a marle, which at the cliff DD is in some places three or four feet above the beach, and when wet is very slippery. A stratum of this marle extends for many miles on the coast, and where it is not sufficiently covered with sand to bear any weight, it is in many places a quag, and dangerous to pass over.

Through this track of land I have described, there are many drains of water, which may be supplied partly from the falling of the rains in wet seasons, and partly from the springs issuing from the hills; and there is reason to suppose, that in a loose soil these drains form channels in a course of time. At the place where the ground has sunk before, and is now sinking, there is a drain from the marle under the sand; and I am of opinion, that the course of the water is in the same direction

as the valley between the hill H and the sinking of the ground AA.

That the sinking of the ground is caused by the foundation being undermined (and I think by water) is evident from the appearance of the ground in the valley. The soil is full of fissures, and resembles an arch, which is sunk down, and has left the two abutments, the hill H and the cliff DD, standing.

As the hill H more than counter-balances the pressure of the sinking ground upon the stratum of wet marle, the consequence is, that the rocks KK, at some yards distance, being only thinly covered with sand, are forced upwards, and become visible, and the wet marle in many places is squeezed through the sand with them.

This appears to me to be the true reason of the sinking of the ground at one place, and the rising of the rocks at another.

That the reverend Mr. SACKETT's account of the sinking of the ground at Folkestone, to the Royal Society, is founded in error, I have not the least doubt, from the present appearance of some of the objects he describes. I am rather at a loss to follow him exactly, as the oldest man in the town of Folkestone (I am told) never heard of the Mooring-rock he mentions.

I think by his description the sinking of the ground must have been in his time at the same place it is now, as Tarlingham-house is not to be seen on the other side of the town.

Admitting this to be the case, there will still be a difficulty respecting the relative situation of each place in explaining what he calls a sketch of the country. But, to explain my meaning more fully, let B (Tab. V.) represent the foot

foot of the hill H in the view, which is upwards of 30 feet high.

CD, the valley between the hill B and the cliff.

DE, the cragged cliff, 60 yards high.

EF, a plain, above a mile long.

FG, a hill of steep ascent, Mr. SACKETTE says near half a mile; but this is much higher than it really is.

GH, the land from the top of the hill to the house near a mile.

I, Tarlingham-house, lying two miles and a half N.N.W. from the rock.

EGH, a line of sight (see Mr. SACKETTE's description of the country).

If Mr. SACKETTE, in the above description of his sketch of the country, had placed each object according to its real situation; and if the effects he has mentioned had been real ones, they would have been truly wonderful, and worthy the attention of the curious investigator of the hidden operations of nature; but I am apprehensive he had but very little better foundation for what he has said than the vague and inconsistent reports of a few ancient fishermen. Tarlingham-house is by Mr. SACKETTE's account situated full as far beyond the hill FG as the width of the plain EF; but how deep the hill has sunk to render the house visible over the top must depend upon the situation of it, *viz.* how much higher it was than the top of the hill.

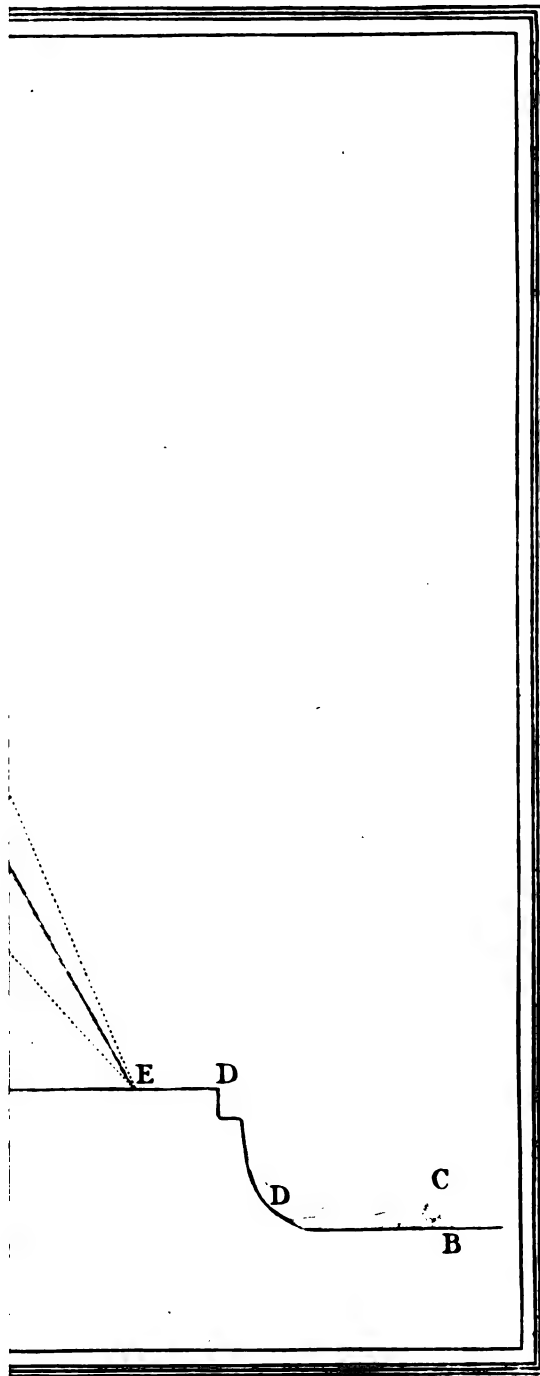
If the hill has sunk only ten feet, there must have been some external evidence of it, such as fissures round the base, and a very steep ascent from the top of it, where the separation happened between it and Tarlingham-house; but there are no traces of any such sinking of the hills.

There is farther proof that Mr. SACKETTE did not examine into the matter himself, but rested what he said upon the report of others; and this is, that Tarlingham-house is not seen over the top of the hill in the line of sight EG, but considerably to the left of it, in the line EI, and clear even of the base of the hill. Besides, a moment's reflection would have told him, that the sinking of the hills could not produce the effects he mentions; for if the ground in the plain was pushed forward by it, it could not be a partial slipping; not only the church, and the whole town, must have been removed, but every object between the base of the hills and the cliff must have been removed out of their place; but I may venture to affirm, there is no proof of this having been done. I should have been drawn into the same or similar errors myself, if I had rested satisfied with the first accounts I received from an ancient fisherman. He told me the same story of the hills sinking in his time, and Tarlingham-house appearing higher than it did since he could remember. In one part of his relation he was right; for I found, upon inquiry, that Tarlingham-house has been taken down, and built upon a much larger scale than formerly, since it has been in the hands of the present proprietor.

If what I have said should not prove satisfactory, I shall be happy in giving you any farther information upon this subject in my power; and am, Sir, &c.

J. LYON.





XI. *Particulars relative to the Nature and Customs of the Indians of North-America. By Mr. Richard M^c Causland, Surgeon to the King's or Eighth Regiment of Foot. Communicated by Joseph Planta, Esq. Sec. R. S.*

Read February 16, 1786.

IT has been advanced by several travellers and historians that the Indians of America differed from other males of the human species in the want of one very characteristic mark of the sex, to wit, that of a beard. From this general observation, the Esquimaux have been excepted; and hence it has been supposed, that they had an origin different from that of the other natives of America. Inferences have also been drawn, not only with respect to the origin, but even relative to the conformation of Indians, as if this was in its nature more imperfect than that of the rest of mankind.

It appears somewhat singular that authors, in deducing the origin both of the Esquimaux and of the other Indians of America from the old world, should never have explained to us how the former came to retain their beards, and the latter to lay them aside. To ascertain the authenticity of this point may perhaps prove of little real utility to mankind; but the singularity of the fact certainly claims the attention of the curious: and as it is impossible to fix any limits to the inferences

rences which may at one time or another be drawn from alledged facts, it must always be of consequence to inquire into the authenticity of those facts, how little interesting soever they may at present appear.

I will not by any means take upon me to say that there are not nations of America destitute of beards ; but ten years residence at Niagara, in the midst of the Six-Nations (with frequent opportunities of seeing other nations of Indians) has convinced me, that *they* do not differ from the rest of men, in this particular, more than one European differs from another : and as this imperfection has been attributed to the Indians of North-America, equally with those of the rest of the Continent, I am much inclined to think, that this assertion is as void of foundation in one region as it is in the other.

All the Indians of North-America (except a very small number, who, from living among white people, have adopted their customs) pluck out the hairs of the beard ; and as they begin this from its first appearance, it must naturally be supposed, that to a superficial observer their faces will seem smooth and beardless. As further proof that they have beards, we may observe, first, that they all have an instrument for the purpose of plucking them out. Secondly, that when they neglect this for any time, several hairs sprout up, and are seen upon the chin and face. Thirdly, that many Indians allow tufts of hair to grow upon their chins or upper lips, resembling those we see in different nations of the old world. Fourthly, that several of the Mohocks, Delawares, and others, who live amongst white people, sometimes shave with razors, and sometimes pluck their beards out. These are facts which are notorious amongst the Army, Indian-Traders, &c. ; and which

are never doubted in that part of the world by any person in the least conversant with Indians: but as it is difficult to transport a matter of belief from one country to another distant one, and as the authors who have maintained the contrary opinion are too respectable to be doubted upon light grounds, I by no means intend to rest the proofs upon what has been said, or upon my single assertion.

I have provided myself with two authorities, which I apprehend may in this case be decisive. One is Colonel BUTLER, Deputy Superintendant of Indian Affairs, well known in the late American war, whose great and extensive influence amongst the Six-Nations could not have been acquired by any thing less than his long and intimate knowledge of them and their language. The other authority is that of THAYENDANEGA, commonly known by the name of Captain JOSEPH BRANT, a Mohock Indian of great influence, and much spoken of in the late war. He was in England in 1775, and writes and speaks the English language with tolerable accuracy. I shall therefore only subjoin their opinions upon this matter, the originals of which I have under their own signatures.

Colonel BUTLER'S.

THE men of the Six-Nation Indians have all beards naturally, as have all the other nations of North-America which I have had an opportunity of seeing. Several of the Mohocks shave with razors, as do likewise many of the Panees who are kept as slaves by the Europeans. But in general the Indians pluck out the beard by the roots from its earliest appearance; and as their faces are therefore smooth, it has been supposed that they were destitute of beards. I am even of opinion, that
if

Mr. Mc CAUSLAND's Observations on the
if the Indians were to practise shaving from their youth, many
of them would have as strong beards as Europeans.

(Signed)

Niagara, April 12, 1784.

JOHN BUTLER.
Agent of Indian Affairs.

Captain BRANT's.

THE men of the Six-Nations have all beards by nature; as have likewise all other Indian nations of North America which I have seen. Some Indians allow a part of the beard upon the chin and upper lip to grow, and a few of the Mohocks shave with razors in the same manner as Europeans; but the generality pluck out the hairs of the beard by the roots as soon as they begin to appear; and as they continue this practice all their lives, they appear to have no beard, or at most only a few straggling hairs which they have neglected to pluck out. I am however of opinion, that if the Indians were to shave they would never have beards altogether so thick as the Europeans; and there are some to be met with who have actually very little beard.

(Signed)

JOS. BRANT THAYENDANEGA.

Niagara, April 19, 1783.

Upon this subject I shall only further observe, that it has been supposed by some, that this appearance of beard on Indians arises only from a mixture of European blood; and that an Indian of pure race is entirely destitute of it. But the nations, amongst whom this circumstance can have any influence, bear so small a proportion to the multitude who are unaffected
by

by it, that it cannot by any means be considered as the cause; nor is it looked upon as such either by Captain BRANT or Colonel BUTLER.

I shall here subjoin a few particulars relative to the Indians of the Six-Nations, which, as they seem not to be well understood even in America, are probably still less known in Europe. My authorities upon this subject, as well as upon the former, are the Indian Captain BRANT and Colonel BUTLER.

Each nation is divided into three or more tribes; the principal of which are called the Turtle-tribe, the Wolf-tribe, and the Bear-tribe.

Each tribe has two, three, or more chiefs, called Sachems; and this distinction is always hereditary in the family, but descends along the female line: for instance, if a chief dies, one of his sister's sons, or one of his own brothers, will be appointed to succeed him. Among these no preference is given to proximity or primogeniture; but the Sachem, during his life-time, pitches upon one whom he supposes to have more abilities than the rest; and in this choice he frequently, though not always, consults the principal men of the tribe. If the successor happens to be a child, the offices of the post are performed by some of his friends until he is of sufficient age to act himself.

Each of these posts of Sachem has a name which is peculiar to it, and which never changes, as it is always adopted by the successor; nor does the order of precedency of each of these names or titles ever vary. Nevertheless, any Sachem, by abilities and activity, may acquire greater power and influence in the nation

than those who rank before him in point of precedence; but this is merely temporary, and dies with him.

Each tribe has one or two chief warriors; which dignity is also hereditary, and has a peculiar name attached to it.

These are the only titles of distinction which are fixed and permanent in the nation; for although any Indian may by superior talents, either as a counsellor or as a warrior, acquire influence in the nation, yet it is not in his power to transmit this to his family.

The Indians have also their *Great Women* as well as their *Great Men*, to whose opinions they pay great deference; and this distinction is also hereditary in families. They do not sit in council with the Sachems, but have separate ones of their own.

When war is declared, the Sachems and great Women generally give up the management of public affairs into the hands of the warriors. It may however so happen, that a Sachem may at the same time be also a chief warrior.

Friendships seem to have been instituted with a view towards strengthening the union between the several nations of the confederacy; and hence *Friends* are called the sinews of the Six-Nations. An Indian has therefore generally one or more *friends* in each nation. Besides the attachment which subsists during the life-time of the two friends, whenever one of them happens to be killed, it is incumbent on the survivor to replace him, by presenting to his family either a scalp, a prisoner, or a belt consisting of some thousands of wampum; and this ceremony is performed by every *friend* of the deceased.

The purpose and foundation of war parties therefore, is in general, to procure a prisoner or scalp to replace the friend or relation of the Indian who is the head of the party. An Indian

dian who wishes to replace a friend or relation presents a belt to his acquaintance, and as many as chuse to follow him accept this belt, and become his party. After this, it is of no consequence whether he goes on the expedition or remains at home (as it often happens that he is a child), he is still considered as the head of the party. The belt he presented to his party is returned fixed to the scalp or prisoner, and passes along with them to the friends of the person he replaces. Hence it happens, that a war party, returning with more scalps or prisoners than the original intention of the party required, will often give one of these supernumerary scalps or prisoners to another war party whom they meet going out; upon which this party, having fulfilled the purpose of their expedition, will sometimes return without going to war.

Abstract of a Register of the Barometer, Thermometer, and
 Rain at London in Rutland, in 1785. By Thomas Barker,
 Esq. Also of the Rain at South Lambeth, in Surrey; and at
 Selbourn and Fyfield, Hampshire. Communicated by Thomas
 White, Esq. F.R.S. Read February 23, 1786.

The severe frost of December, 1784, broke early in January, and was all gone before the middle; and the most open part of this sharp winter followed it, being misty or thick and warm very wet air; but the last day of January another frost set in, which, though not so steady as the former, was sometimes very severe, and did not go away till near the middle of March: and this winter, particularly the former frost about December 10, was much severer in the south of England than here, and greater signs of destruction by it were seen among the trees and plants there. From the breaking of the frost till April 4, was chiefly frosty mornings, and sometimes in the shade all day, so that, if you count the number of frosty days, I do not know that any winter had more, though I have known several longer frosts, and more steady, and some few more severe.

From April 5, the weather began to mend, was tolerably pleasant, and things came on gradually; yet not without some frosty mornings, even in May. The seed time began late, but was without hindrance; and there having been very little rain since the frost, it harrowed remarkably fine, and the lands and roads were uncommonly dusty. The corn came up very well, except the late sown, some of which, especially in the south of England, lay dry till June; for it continued a remarkably dry time all spring, so that the grass was very short, and hay very scarce; yet the grain continued particularly fine-coloured, and eared very well, though some of the winter corn was rather thin; yet that was much mended by some refreshing showers in May and June, which were enough to freshen things, though not to make much grass: and during this drought there were great numbers of little whirlwinds, sometimes several in a day.

The weather began to be showery the middle of July, and several great rains; and after August 3d it was more frequent, but less at a time. This made plenty of good grass, but was very troublesome for the harvest, which was got in slowly, and with loss, but came out again full as well as could be expected. The wheat was remarkably full-eared. The barley good, except the late sown, which never ripened; and some too hastily carried in harvest. The birds of passage went away rather early this year: almost all the Swifts were gone in July, and most of the Swallows and Martins in September; the last were August 7, and October 12. It continued very showery till near the middle of October; after which the autumn was pretty fine, and less wet than before, yet enough to make it very dirty when the sun lost its power in December; and the winter began for the most part open and pleasant, till a frost and large snow at Christmas, which grew severer to the end of the year.

On the Variations of Seasons.

Measuring the rain for a few years will not shew completely the general quantity of rain which falls in any place; for there is a very great difference at different periods of time. If I had measured the rain at Lyndon only in the four years 1740, 1741, 1742, 1743, the mean would have been found to be only 16½ inches in a year; yet they were not all complained of as dry summers. 1740 was cold and dry till July 30. The
spring

spring 1741 was cold and dry, the summer hot, dry, and burning till the beginning of September; then ten days wet and very warm again, being the finest autumn for grass ever known. 1742 was a showery summer, and 1743 wet in the middle; but then the winters were dry, so that the quantity of rain upon the whole was small. 1741 to 1750 the mean was 18½ inches. 1741 and 1750 were hot, dry, and burning, 1750 being the hottest year I have known. The intermediate years were neither very wet nor very dry; and this was the most plentiful and cheapest time for corn of any ten years I remember; for grain oftener fails in England from too much wet than too little. 1751 to 1760 the mean year was 22¼. 1760 was hot, dry, and burning; but several of the summers were wet, and the crops not so plentiful. Three wet summers together, 1754, 1755, and 1756, were a time of scarcity, and we have had more failing crops since that time than before it. From 1761 to 1770 there was 23¼ in a year. 1762 was hot, dry, and burning; and 1765 cold and dry; but several years were wet, 1763 and 1768 remarkably so; and of those ten years several had failing crops, and some had great snows. There was a great change of the seasons at 1763; for I have had more rain since that time than I had before it in the proportion of 5 to 4. From 1770 to 1780 there was at a mean 26 inches. 1771 was dry, and 1778 and 1779 were hot, yet not without fits of rain; and most of the other years were wet, and some great snows. 1773, 1774, and 1775, were so wet that there came 32 inches in a year, which is nearly double what there was from 1740 to 1743. In twelve months, from October 1773, to September 1774, there came 39,390 inches of rain, which is nearly a Lancashire year. And in

one month, September 1774, there was 8 inches: this was in barley and pease harvest, and for three weeks together not a load could be carried in. By the above state of the case it appears, that, for four successive periods of ten years, the quantity of rain has been increasing each time.

XIII. *An Account of Experiments made by Mr. John Mc Nab, at Henley House, Hudson's Bay, relating to freezing Mixtures. By Henry Cavendish, Esq. F. R. S. and A. S.*

Read February 23, 1786.

IN my observations on Mr. HUTCHINS's Experiments, printed in the LXXIII^d volume of the Philosophical Transactions, I gave my opinion concerning the cause of the cold produced by mixing snow with different liquors. As there were some circumstances, however, which seemed to form a difficulty in the way of this opinion, I was desirous of having further experiments made on the subject; and at the same time I thought that, by proper management, a greater degree of cold might be produced than had hitherto been done. On mentioning the experiments I wished to have made to Mr. HUTCHINS, he very obligingly desired Mr. Mc NAB, Master at Henley-House, to try them; who was so good as to undertake the business, and has executed it in the most satisfactory manner; as he has not only taken great pains, but has shewn the utmost attention and accuracy, in observing and relating all the phænomena which occurred, and has manifested great judgement in frequently adapting the manner of trying the experiments to appearances which occurred in former ones, to which we are indebted for great part of the most curious facts in this paper. His endeavours have also been attended with much success, as he has not only shewn many remarkable circumstances relating to the freezing of the nitrous

and vitriolic acids, and the phænomena of freezing mixtures; but has also produced degrees of cold greatly superior to any before known.

1. In the above-mentioned Paper I said, that the cold produced by mixing spirit of nitre with snow, is owing to the melting of the snow; and that in all probability there is a certain degree of cold, in which spirit of nitre is so far from dissolving snow, that it will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials, before mixing, is equal to this, no additional cold can be produced. A circumstance, however, which at first sight seems repugnant to this opinion, occurred in an experiment of FAHRENHEIT's for producing cold by a mixture of spirit of nitre and ice; namely, that the acid, which had been repeatedly cooled by different frigorific mixtures, was found frozen before it was mixed with the ice; notwithstanding which, cold was produced by the mixture. Professor BRAUN also found, that cold was produced by mixing frozen spirit of nitre with snow. On consideration, however, this appeared by no means inconsistent with the opinion there laid down, as there was great reason to think, that the freezing of the acid was of a different kind from that considered in the above-mentioned Paper, and that it did not proceed from the watery part separating from the rest and freezing; but that the whole acid, or perhaps the more concentrated part, froze; in which case it would not be extraordinary that the acid should dissolve more snow, and produce cold.

2. To clear up this point, I sent to Hudson's Bay a bottle of spirit of nitre, of nearly the same strength as FAHRENHEIT's; and desired Mr. M^c NAB to expose it to the cold, and, if it froze, to ascertain the temperature, and decant the fluid part into another

another bottle, and send both home to be examined, as it would thereby be known, whether it was the whole acid, or only the watery part, which froze, . For the same purpose also I sent some dephlogisticated spirit of nitre of the same strength, and also some strong oil of vitriol. I also sent some spirit of nitre and spirit of wine, both diluted with so much water, that it was expected, that with the cold of Hudson's Bay they would suffer the first kind of congelation; that is, their watery part would freeze, and thereby make the difference between the two kinds of freezing more apparent.

3. In the same Paper I say, " That on adding snow gradually to some of the spirit of nitre used by Mr. HURCHINS, " I found, that the addition of a small quantity produced heat " instead of cold; and it was not until so much was added as to " increase the heat from 28° to 51° , that the addition of more " snow began to produce cold; the quantity of snow required " for this purpose being pretty exactly one quarter of the " weight of the spirit of nitre, and the heat of the snow and " air of the room, as well as the acid, being 28° . The reason " of this is, that a great deal of heat is produced by mixing " water with spirit of nitre, and the stronger the spirit is, the " greater is the heat produced. Now it appears from this " experiment, that before the acid was diluted, the heat " produced by its union with the water formed from the melted " snow was greater than the cold produced by the melting of " the snow; and it was not till it was diluted by the addition " of one quarter of its weight of that substance, that the cold " generated by the latter cause began to exceed the heat generated by the former. From what has been said, it is evident, that the cold of a freezing mixture, made with the " undiluted acid, cannot be quite so great as that made with

“ the same acid, diluted with a quarter of its weight of water;
 “ supposing the acid and snow to be both at 28° of heat; and
 “ there is no reason to think, that the event will be different if
 “ they are colder; for the undiluted acid will not begin to
 “ generate cold, until so much snow is dissolved as to increase
 “ its heat from 28° to 51° , so that no greater cold will be
 “ produced, than would be obtained by mixing the diluted acid
 “ heated to 51° with snow of the heat of 28° . This method
 “ of adding snow gradually to an acid, is much the best way
 “ I know of finding what strength it ought to be of, in order
 “ to produce the greatest effect possible.”

As it seemed likely that, by following this method, a greater degree of cold might be produced than had been done hitherto; I sent three other bottles of spirit of nitre and oil of vitriol, all three diluted, but not so much so, but that I thought they would require a little further dilution, in order to reduce them to their properest degree of strength. I also sent a bottle of highly rectified spirit of wine, and a mixture of equal quantities of the above-mentioned common spirit of nitre and oil of vitriol; and desired Mr. M^r NAB to find what degree of cold could be produced by mixing them with snow, after having first reduced them, in the above-mentioned manner, to their best degree of strength*.

He was also desired to ascertain how much snow he added; for as their strength was determined before they were sent out, it would thereby be known what was the best strength of these liquors for frigidific mixtures.

* This might have been done at home; but I thought it not unlikely that the strength found this way might differ, in some measure, according to the heat in which the experiment was tried.

All these bottles were numbered with a diamond; and as I shall sometimes distinguish them by these numbers, and as it may be of use to those who may consult the original, I have added the following list of these bottles, with their contents.

N ^o	Liquors mentioned in Art. 3.	Weight of marble which they dissolve.	Specific gravity at 60° of heat.
168	Spirit of nitre,	,582	1,4371
27	Dephlogisticated spirit of nitre,	,53	1,4040
103	Diluted oil of vitriol,	,654	1,5596
28	Equal weights of N ^o 168. and N ^o 103.	- - -	- - -
8	Very highly rectified spirit of wine,	- - -	,8195
Liquors mentioned in Art. 2.			
151	Strong oil of vitriol,	,98	1,8437
142	Spirit of nitre,	,525	1,4043
139	Some of the same diluted with twice its weight of water,	- - -	- - -
141	Dephlogisticated spirit of nitre,	,53	1,4033
143	Some of the same spirit of wine as in N ^o 8. diluted with 1½ its weight of water,	- - -	- - -
72	Diluted oil of vitriol for comparing the thermometers,	,629	- - -
171	Oil of vitriol of about the usual strength, but the exact strength not known, intended to refresh the former when too weak.		

4. Professor BRAUN says, that by mixtures of snow and spirit of nitre he sunk thermometers filled with oil of saffras, and some other essential oils, to -100° or -124° ; and that, by the same means, he sunk thermometers filled with the highest rectified spirit of wine to -148° . Though there seemed great reason to think, from Mr. HUTCHINS's experiments, that there must be some mistake in this; yet, as it was possible that the essential oils, and even spirit of wine of a strength much different from that with which Mr. HUTCHINS's thermometers were filled, might follow a considerably different progression in their contraction

contraction by great degrees of cold, I sent a thermometer filled with oil of saffrafras, and two others with spirits of wine. One of these last was filled with the highest rectified spirits I could procure, its specific gravity at 60° of heat being ,8185; the other was intended to be filled with common spirits, though from circumstances I am inclined to suspect *that* also to have been filled with the best spirits. Besides these, there was sent a mercurial thermometer, accurately adjusted, according to the directions of the Committee of the Royal Society, printed in the LXVIIth volume of the Transactions; and also the two spirit thermometers used by Mr. HUTCHINS, which were filled with spirits whose specific gravity was ,8247.

5. These thermometers were compared together by exposing them to the cold, with their balls immersed in a glass vessel filled with diluted oil of vitriol. They were at times also compared in cold more violent than the natural cold of the climate, by adding snow to the acid in which they were tried, in which case care was taken to keep the mixture frequently stirred. Oil of vitriol was recommended for this purpose, as a fluid which would most likely bear any degree of cold without freezing, and whose natural cold might be much increased by the addition of snow. It seems to have answered the purpose very well, and not to have been attended with any inconvenience.

During the first comparison of these thermometers, a whitish globule, such as those which appear in frozen oil, was observed in the tube of the thermometer filled with oil of saffrafras. This appearance of congelation did not much increase; but two days after a large air bubble was found in its ball, which prevented Mr. MC NAB from making further observations with it.

It

It is well known, that spirit of wine expands more by a given number of degrees of a mercurial thermometer in warm temperatures than in cold ones; and this inequality, as might be expected, was less in the stronger spirit than in the weaker, but the difference was inconsiderable. The oil of saffraas also had some of this inequality, but much less. It however appears to be by no means a proper fluid for filling thermometers with. No appearance was observed which indicates any considerable irregularity in the contraction of spirits of wine in intense cold, or which renders it probable, that thermometers filled therewith could be sunk by a mixture of snow and spirit of nitre to a degree near approaching to that mentioned by Professor BRAUN.

6. Mr. M^c NAB in his experiments sometimes used one thermometer and sometimes another; but in the following pages I have reduced all the observations to the same standard; namely, in degrees of cold less than that of freezing mercury I have set down that degree which would have been shewn by the mercurial thermometer in the same circumstances; but as that could not have been done in greater degrees of cold, as the mercurial thermometer then becomes of no use, I found how much lower the mercurial thermometer stood at its freezing point, than each of the spirit thermometers, and increased the cold shewn by the latter by that difference.

On the common and dephlogisticated Acids of Nitre.

The following experiments shew, that both these acids are capable of a kind of congelation, in which the whole, and not merely the watery part, freezes. Their freezing point also differs

differs greatly according to the strength, and varies according to a very unexpected law. Like water too they bear being cooled very much below their freezing point before the congelation begins, and as soon as that takes place, immediately rise up to the freezing point.

7. On the morning of Feb. 1. the common and dephlogisticated spirits of nitre, N^o 142 and 141, whose specific gravities were 1,4043 and 1,4033, were found clear and fluid, the cold of the air at that time being -47° . They also bore being shook without any alteration; but on taking out their stoppers, both of them in a few minutes began to freeze, the congelation beginning by a white appearance at top, which gradually spread to the bottom; and they became so thick as not to move on inclining the phial. For want of a thermometer whose ball reached far enough below its scale, Mr. Mc NAB was not able to determine their cold while in the bottle; but in somewhat more than an hour's time, the frozen acid had so much subsided as to admit of his pouring a little fluid matter out of each into a glass with a thermometer in it*; whereby the cold of the common spirit of nitre was found to be $-31^{\circ}\frac{1}{2}$, and that of the dephlogisticated acid -30° , the temperature of the air being -41° . Each of these decanted liquors, at the time their temperature was tried, was full of small *spicula* of ice: they were then put into phials well stopped, and they, as well as the undecanted liquors, sent home to be examined. The decanted part of the common

* It may be asked, why it was more possible to decant any liquor at this time than at first, as the acid was all the while exposed to a cold much below the freezing point? The reason in all probability is, not that any part of the ice first formed dissolved, but that the small filaments into which it^e ~~not~~ collected together, and in some measure subsided to the bottom.

spirit of nitre dissolved, 535 of its weight of marble, and the undecanted part, 523; for which reason I shall call the strength of the former, 535, and that of the latter, 522; which mode of reckoning is observed in the remainder of this Paper. The strength of the decanted part of the dephlogisticated acid was, 56, and that of the undecanted part, 528; so that it appears that in each of these acids the unfrozen part was a little stronger than the frozen part. It is remarkable, that in the common spirit of nitre, the decanted part, though stronger than the other, was paler coloured and less fuming.

8. On Dec. 21, the temperature of the air being -28° , some dephlogisticated spirit of nitre (N^o 27.) of nearly the same strength as the former acid, was poured into a jar, in order to be diluted with snow, as recommended in Art. 2. Immediately after it was decanted, it began to freeze, in the same manner as before described, except that a less portion of it seems to have congealed: its temperature, tried by dipping a thermometer into it, was -19° , where it remained stationary for many minutes; it was then diluted with snow, as will be mentioned in Art. 14. whereby its strength was reduced to 434.

9. On Dec. 29th, this diluted acid was completely melted, and half of it poured into a jar with a ground stopper, and both portions exposed to the air. In the morning they were perfectly fluid; but on taking the stopper out of the jar, and dipping in it a thermometer, the acid immediately froze, beginning by forming a white coat round the ball of the thermometer, which gradually spread through the whole fluid; and at the same time the thermometer rose till it stood stationary at -5° . The cold of the acid before it began to freeze must have been about $-30^{\circ}\frac{1}{2}$, that being the temperature of a

glafs of vitriolic acid ftanding near it; but the thermometer which was dipped into it was five or fix degrees colder, which feems to be the caufe of the congelation beginning round the ball.

In the afternoon a thermometer was dipped into the other half of the acid, where, as the weather had grown lefs cold, it ftood above a minute at -25° , without freezing; then, however, the acid froze, with the fame appearance as in the morning, and at the fame time the thermometer rofe to -4° , and became ftationary.

This acid, being left in the air with the thermometer in it, was found in the evening at -45° ; it however was not intirely frozen, being only thick as an unguent, which fhews that the unfrozen part muft have been of a different ftrength from the frozen part; but it does not appear whether ftonger or weaker. The next morning it was frozen folid, though the cold was only half a degree greater.

On Jan. 16th, this acid was again tried in the fame manner; it then fuffered a thermometer, whole ball had been previously warmed in the hand, to be dipped into it, and remain there feveral minutes without freezing, though its temperature was -35° . But on lifting up the thermometer, a drop fell from its ball into the acid, which immediately fet it a freezing, and it rofe up to $-4^{\circ}\frac{1}{2}$.

10. On Dec. 22d, the fpirit of nitre (N^o 168.) which a few days before had been diluted with fnow, fo as to be reduced to the ftrength of ,411, was divided into two equal parts, and expofed to the cold. On Dec. 29th, when the temperature of the air was $-17^{\circ}\frac{1}{2}$, one of thefe parts was found beginning to freeze; the other was fluid, but began to freeze on dipping in a thermometer; the thermometer in both kept ftationary at $-1^{\circ}\frac{1}{2}$.

-1°½. The latter was twice re melted and exposed to the cold, and both times the temperature of the frozen acid came out the same as before.

11. The white colour of the ice in these experiments seems owing only to its consisting of very slender filaments; for in some cases, where it froze slower, and where, in consequence, it shot into larger solid masses, they were transparent, and of the same colour as the acid itself. By the continuance of a sufficient cold, the acid, which by hasty freezing put on the white appearance, would become hard solid ice, but yet still retained its white appearance, owing perhaps to the filaments first shot consisting of an acid differing in strength from that which froze afterwards, and filled up the interstices.

In all these experiments, whether the ice was formed into minute filaments or solid masses, still, whenever there was a sufficient quantity of fluid matter to admit of it, they constantly subsided to the bottom; a proof that the frozen part was heavier than the unfrozen. The difference indeed is so great, that in one case where it froze into solid crystals on the surface, these crystals, when detached by agitation, fell with force enough to make a tinkling noise against the bottom of the glass.

These acids contract very much on freezing. Whenever the acid is frozen solid, the surface, instead of being elevated in ridges, like frozen water, is depressed and full of cracks. In one experiment Mr. M^r NAB, after a glass almost full of acid was nearly frozen, filled it to the brim with fresh acid; and then, after it was completely frozen, the surface was visibly depressed, with fissures one-eighth of an inch broad, extending from top to bottom. It is this contraction of the acid, in freezing which makes the frozen part subside in the fluid

part; as it was found, in the undiluted acid, that the latter consisted of a stronger, and consequently heavier, acid than the former. But still the subsidence of the frozen part shews, that the ice is not mere water, or even a very dilute acid; which indeed was proved by the examination of the liquors sent home.

The ninth and tenth articles shew, that though the acids bear being cooled greatly below the freezing point, without any congelation taking place, yet as soon as they begin to freeze they immediately rise up to their freezing point; and this point is always very nearly, if not exactly, the same in the same acid; for those acids were frozen and melted again three or four times, and were cooled considerably more below the freezing point in one trial than another, and yet as soon as they began to freeze the thermometer immersed in them constantly rose nearly to the same point.

The quantity which these acids will bear being cooled below the freezing point, without freezing, is remarkable. The diluted spirit of nitre, whose freezing point is $-1^{\circ}\frac{1}{4}$, bore being cooled to near -39° , without freezing, that is, near 37 degrees below its freezing point. The diluted dephlogisticated spirit of nitre, whose freezing point is -5° , bore cooling to -35° ; and the dephlogisticated spirit of nitre (141) whose true freezing point is most likely -19° (*see next article*) bore being cooled to -49° : perhaps too they might have borne to be cooled considerably lower without freezing, but how much does not appear. It must be observed, however, that the same diluted spirit which at one time bore being cooled to -39° , at another froze, without any apparent cause, when its cold was certainly less than -30° , and most likely not much below -18° .

12. The freezing point differs remarkably, according to the strength of the acid. In the diluted dephlogisticated and common spirit of Art. 7. and 8. the freezing point was -5° and $-1^{\circ}\frac{1}{2}$. In the dephlogisticated and common spirit of Art. 5. the decanted parts of which were stronger than the foregoing in scarcely so great a proportion as that of four to three, it seemed to be -30° and $-31^{\circ}\frac{1}{2}$. It may indeed be suspected, that as this point was determined only by pouring a small quantity of the acid into a glass, at a time when the air and glass were much colder than the acid, these decanted liquors might be cooled by the air and glass, and thereby make the freezing point appear lower than it really was: but I do not think this could be the case; for as the decanted liquors were full of small filaments of ice, they could hardly be cooled sensibly below their freezing points without freezing; and any cold, communicated to them by the air or glass, would serve only to convert more of them into ice, without sensibly increasing their cold: so that I think this experiment determines the true freezing point of their decanted part; but it must be observed, that as the decanted part was rather stronger than the rest, it is very possible that the freezing point of the undecanted part might be considerably less cold.

A circumstance which might incline one to think, that the way by which the freezing point was determined in this experiment is defective is, that the freezing point of the dephlogisticated acid N^o 27. though nearly of the same strength as that last mentioned, but rather stronger, was much less low, being only -19° . But I have little doubt that the true reason of this is, that in the former acid the strength of the decanted part, which is the part whose freezing point was tried, was found to be at least $\frac{1}{16}$ greater than that of the whole mass; whereas
in

in N^o 27. the fluid part was in all probability not sensibly stronger than the whole mass; for as N^o 27. was cooled only seven degrees below the freezing point, and its temperature was tried soon after its beginning to freeze, not much of the acid could have frozen; whereas the other was cooled 15 degrees below its freezing point, and was exposed for an hour or two to an air not much less cold, in consequence of which a considerable part of the acid must have frozen; so that in all probability the acid, whose freezing point was found to be -30° , was in reality $\frac{1}{8}$ part stronger than that whose freezing point was -19° .

If this reasoning be just, the freezing point of these acids is as follows:

		Freezing point.
Dephlogificated spirit of nitre, whose strength =	,56	-30°
	,53	-19
	,437	$-4\frac{1}{2}$
Common spirit of nitre, whose strength =	,54	$-31\frac{1}{2}$
	,411	$-1\frac{1}{2}$

On the Phænomena observed on mixing Snow with these Acids.

13. On Dec. 13, snow was added to the spirit of nitre N^o 168, as recommended in Art. 2. The snow was put in very gradually, and time was taken to find what effect each addition had on the thermometer and mixture, before more was added. The temperature of the acid before the mixture was -27° , and each addition of snow raised the thermometer a little, till it rose to $-1^{\circ}\frac{1}{2}$; after which the next addition made it sink to -2° , which shewed that sufficient snow had then been added. The quantity

quantity of snow used was pretty exactly $\frac{1}{4}$ of the weight of the acid, the weight of the acid being 13 oz. so that the strength of the diluted acid was reduced to ,411.

The acid before the addition of snow had no signs of freezing, its temperature being in all probability much above its freezing point; yet the snow did not appear to dissolve, but formed thin white cakes, which however did not float on the surface, but fell to the bottom, and when broke by the spatula formed a gritty sediment; so that it appears, that these cakes are not simply undissolved snow, but that the adjoining acid absorbed so much of the snow in contact with it, as to become diluted sufficiently to freeze with that degree of cold, and then congealed into these cakes. The quantity of congealed matter seems to have kept increasing till the end of the experiment.

14. On Dec. 21, an experiment was made in the same manner with the dephlogisticated spirit of nitre N° 27. The acid began to freeze in pouring it into the jar in which the mixture was to be made, and stood stationary there at -19° , as related in Art. 6.; so that the liquor at the beginning of the experiment was white and thick, which made the effect of the addition of the snow less sensible. However, the congealed matter constantly subsided to the bottom, and the quantity seems to have continued increasing to the end of the experiment. The heat of the mixture rose to -4° before cold began to be produced, and the quantity of snow added was $\frac{2\frac{1}{2}}{10}$ of that of the acid, so that the strength of the acid was reduced to ,437 by the dilution.

A very remarkable circumstance in this experiment is, that the acid, while the snow was adding, first became of a yellowish,

lowish, and afterwards of a greenish or bluish hue. This colour did not go off by standing, but continued at least ten days, during which time the acid constantly kept that colour, except when by hasty freezing it shot into small filaments, in which case it put on the white appearance which these acids always assumed under those circumstances; but once that by gradual freezing it shot into transparent ice, this ice was of a bluish colour.

It is difficult to conceive what this colour should proceed from. Spirit of nitre is well known to assume this colour when much phlogisticated and properly diluted; but one does not see why it should become phlogisticated by the addition of the snow, and still less why the dephlogisticated acid should become more phlogisticated thereby than the common acid did; for though it is not extraordinary, that a process not capable of producing any increase of phlogistication in the common acid, should make this as much phlogisticated as that, yet it is very extraordinary that it should make it more so. No notice is taken of any effervescence or discharge of air while it was assuming this colour, nor was it observed that it became more smoking thereby, or that the top of the phial in which it was kept became full of red fumes, as might naturally be expected if it was rendered much phlogisticated. These are circumstances which, considering Mr. M^c NAB's great attention to set down all the phænomena that occurred, I should think would hardly have been omitted if they had really happened.

15. It is remarkable, that in both these experiments the addition of snow produced heat, until it arrived pretty exactly at what was found to be the freezing point of the diluted acid; but that as soon as it arrived at that point, the addition of more snow began to produce cold. This can hardly be owing merely

merely to accident, and to both acids having happened to be of that precise degree of heat before the experiment began, that their heat after dilution should coincide with the freezing point answering to their new strength. The true cause seems to be as follows. It will be shewn in Art. 16. and 17. that the freezing point of these acids, when diluted as in the foregoing experiments, is much less cold than when they are considerably more diluted; and it was before shewn to be much less cold than when not diluted; so that there must be a certain degree of strength, not very different from that to which these acids were reduced by dilution, at which they freeze with a less degree of cold than when they are either stronger or weaker. Now in these experiments, the temperature of the liquors before dilution was below this point of easiest freezing, and a great deal of the acid was in a state of congelation all the time of dilution; the consequence of which is, that when they were diluted to the strength of easiest freezing, they would also be at the heat of easiest freezing; for they could not be below that point, because, if they were, so much of the acid would immediately freeze as would raise them up to it; and they could not be above it, for, if they were, so much of the congealed acid would dissolve as would sink them down to it. After they were arrived at this strength of easiest freezing, the addition of more snow would produce cold, unless this strength be greater than that at which the addition of a small quantity of snow begins to produce cold; but even were this the case, heat would not be produced, but the temperature of the acids would remain stationary until they were so much diluted that the addition of more snow should produce cold. So that, in either case, the heat of the acids, at the time that the addition of fresh snow began to produce cold, must be that of easiest

freezing; and consequently, as this heat was found to coincide very nearly with the freezing point of these acids, after dilution, it follows that their strengths at that time could differ very little from the strength of easiest freezing.

If the temperature of the liquors at the beginning of the experiment had been above the point of easiest freezing, none of the acid would have congealed during the dilution, and nothing could have been learnt from the experiment relating to the point of easiest freezing; but the heat would have kept increasing, till the acid was diluted to that degree of strength at which the cold produced by the dissolving of the snow was just equal to the heat produced by the union of the melted snow with the acid*; after which the addition of more snow would begin to produce cold. When I recommended this method of finding the best strength of spirit of nitre for producing cold, by the addition of snow, I was not aware of any impediment from the freezing of the acid, in which case it would have been a very proper method; but on account of this circumstance it can hardly be considered as such, except when the cold of the acid at setting out is less than that of easiest freezing.

In the dephlogisticated spirit of nitre the freezing points answering to the strength of ,434, ,53, and ,56, were said to be $-4^{\circ}\frac{1}{2}$, -19° , and -30° ; and the differences of -30° and -19° from $-4^{\circ}\frac{1}{2}$ are to each other very nearly in the duplicate ratio of ,126 and ,096, the differences of the corresponding strengths from ,434; which, as ,434 is the strength of easiest freezing, is the proportion that might naturally be

* In the experiment related in my observations on Mr. HUTCHINS's Experiments, this strength was rather greater than that of easiest freezing: but whether it is so in degrees of cold exceeding that in which my experiment was tried, does not appear.

expected, and consequently serves in some measure to confirm the reasoning in this and the 12th Article.

16. After Mr. M^c NAB had diluted these acids as above-mentioned, he divided each of them into two parts, and tried what degree of cold could be produced by mixing them with snow. On January 15th, one of these parts of the common spirit of nitre was tried. It was fluid when the experiment began, though its temperature, as well as that of the snow, was $-21^{\circ}\frac{1}{4}$; but on adding snow it immediately began to freeze, and grew thick, and its heat increased to $-2^{\circ}\frac{1}{4}$; but by the addition of more snow it quickly sunk again, and at last got to $-43^{\circ}\frac{1}{4}$. During the addition of the snow, the mixture grew thinner, and by the time it arrived at nearly the greatest degree of cold, consisted visibly of three parts: the lowest part, which consisted of frozen acid, was white and felt gritty; the upper part, which occupied about an equal space, was also white, but felt soft, and must have consisted of unmelted snow; the other part, which occupied by much the smallest space, was clear and fluid. The quantity of snow added was about $\frac{1}{3}$ of the weight of the acid, and consequently its strength was reduced to .243.

Though snow was added to the acid in this experiment as long as, and even longer than, it produced any increase of cold, yet some days after, on adding more snow to the mixture, while it was fluid, and of the temperature of $-45^{\circ}\frac{1}{4}$, the cold was increased to $-44^{\circ}\frac{1}{4}$, or 1 degree lower than before. Mr. M^c NAB did not perceive the snow to melt, though in all probability some must have done so, or no cold would have been produced.

The cause of this seems to be, that in the preceding experiment the congealed part of the acid was stronger than the

fluid

fluid part; so that, though the fluid part was not strong enough to dissolve snow in a cold greater than $-43^{\circ}\frac{1}{2}$, yet the whole acid together was strong enough to do it in a cold one degree greater.

A circumstance occurred in the last experiment which I cannot at all see the reason of; namely, a small part of the acid being poured into a saucer, before the addition of the snow, it was in an hour's time changed into solid ice, though the cold of the air, at the time the acid was poured out, was only $-41^{\circ}\frac{1}{2}$, and does not seem to have increased during the experiment.

17. On December 30, the other half of the same acid had been tried in the same manner; at the beginning of the experiment not more than one-ninth part of the acid was fluid, the rest solid clear ice; its temperature was $-34^{\circ}\frac{1}{2}$, and that of the snow nearly the same; the greatest degree of cold produced was $-42^{\circ}\frac{1}{2}$; and the quantity of snow employed was about one-eighteenth of the weight of the acid; so that the strength of the mixture was ,38. The freezing point of the acid thus diluted appears to be about $-45^{\circ}\frac{1}{2}$; for by the increase of warmth during the day-time, most of the congealed matter dissolved; but in the evening it began to freeze again, so as to become thicker, its temperature being then $-45^{\circ}\frac{1}{2}$; and the next morning it was frozen solid, its cold being one degree greater.

18. On December 12, the diluted spirit of nitre N^o 139, whose strength was ,175, was found frozen, its temperature being -17° . The fluid part, which was full of thin flakes of clear ice, and was of the consistence of syrup, was decanted into another bottle, and sent back. Its strength was ,21, and was greater than that of the undecanted part in the proportion of ,21 to ,16; so that, as not much of the undecanted part was

really congealed, the frozen part of the acid must have been much weaker than the rest, if not mere water. Accordingly, during the melting of the undecanted part, the frozen particles swam at top. Mr. Mc NAB added snow to a little of the decanted liquor, but it did not dissolve, and no increase of cold was produced.

19. From these experiments it appears, that spirit of nitre is subject to two kinds of congelation, which we may call the aqueous and spirituous; as in the first it is chiefly, if not entirely, the watery part which freezes, and in the latter the spirit itself. Accordingly, when the spirit is cooled to the point of aqueous congelation, it has no tendency to dissolve snow and produce cold thereby, but on the contrary is disposed to part with its own water; whereas its tendency to dissolve snow and produce cold, is by no means destroyed by being cooled to the point of spirituous congelation, or even by being actually congealed. When the acid is excessively dilute, the point of aqueous congelation must necessarily be very little below that of freezing water; when the strength is ,21, it is at -17° , and at the strength of ,243, it seems, from Art. 16. to be at $-44^{\circ}\frac{1}{2}$. Spirit of nitre, of the foregoing degrees of strength, is liable only to the aqueous congelation, and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold, when the strength is ,411, in which case the freezing point is at $-1^{\circ}\frac{1}{2}$. When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of ,411 than the unfrozen part; it certainly does so, when the strength is greater than ,411, and there is little doubt but what it does so in the other case. At the strength of ,54 the point of spirituous congelation.

20. In trying the first half of the dephlogisticated spirit of nitre, the cold produced was $-44^{\circ}\frac{1}{2}$. The acid was fluid before the addition of the snow, and of the temperature of -36° , but froze on putting in the thermometer, and rose to -5° , as related in Art. 7.

In trying the second part, the acid was about 0° before the addition of the snow, and therefore had no disposition to freeze. The cold produced was $-42^{\circ}\frac{1}{2}$.

As the quantity of snow added in these experiments was not observed, they do not determine any points of aqueous or spirituous congelation in this acid; but there is reason to think, that these points are nearly the same as those of common spirit of nitre of the same strength, as the cold produced in these experiments was nearly the same as that obtained by the common spirit of nitre.

* The point of easiest freezing.

On the Vitriolic Acid.

21. On December 12, the strong oil of vitriol N° 151. was found frozen, and was nearly of the colour and consistence of hogs-lard. Its temperature, found by pressing the ball of a thermometer into it, was -15° , and that of the air nearly the same; but in the night it had been exposed to a cold of -33° . It dissolved but slowly on being brought into a warm room, and was not completely melted before it had risen to $+20^{\circ}$, and even then was not very fluid, but of a syrupy consistence. During the progress of the melting, the congealed part sunk to the bottom, as in spirit of nitre; and many air bubbles separated from the acid, which, when it was completely melted, formed a little froth on the surface. As soon as it was sufficiently melted to admit of it, which was not till it had risen to the temperature of $+10^{\circ}$, the fluid part was decanted, and both were sent home to be examined.

It is remarkable, that the frozen part did not intirely dissolve until the temperature was so much increased. This would incline one to think, that the frozen part must have differed in some respect from the rest, so as to require much less cold to make it freeze; but yet I could not find that the strength of the decanted part differed sensibly from the rest.

It appeared by another bottle of oil of vitriol, which also froze by the natural cold of the air, that this acid, as well as the nitrous, contracts in freezing.

22. On December 21, when the weather was at -30° , the vitriolic acid N° 103. was diluted with snow, as directed in Art. 3. The snow dissolved immediately, and no signs of congelation appeared during any part of the process. The temperature

temperature of the acid rose only one degree before it began to sink, and the weight of the snow added was only $\frac{1}{15}$ of that of the acid, so that its strength was reduced thereby to ,605; which is therefore the best degree of strength for producing cold by the addition of snow, when the degree of cold set out with is -30° . This strength is one-fifteenth part less than what I found myself, by a similar experiment, when the temperature of the acid was $+27^{\circ}$; which shews, that the best degree of strength is rather less, when the degree of cold set out with is great than when small, but that it does not differ much.

23. The acid thus diluted was divided into two parts, and the next day Mr. M^c NAB tried what degree of cold could be produced by adding snow to one of them. The temperature of the air at the time was -39° , and the mixture sunk by the process to $-55^{\circ}\frac{1}{2}$. The snow dissolved readily, and the mixture did not lose much of its fluidity until it had acquired nearly its greatest degree of cold, nor did any congealed matter sink to the bottom in any part of the process. The quantity of snow added was about $\frac{1}{10}$ of the weight of the acid, so that the strength of the mixture was about ,325.

24. On January 1, thin crystals of ice were found diffused all through this mixture, the temperature of the air being $-51^{\circ}\frac{1}{2}$, but that of the liquor was not tried. As this congelation must have been of the aqueous kind, and seems to have taken place at the temperature of $-51^{\circ}\frac{1}{2}$, it should follow, that this acid had no power of dissolving snow in a cold of $51^{\circ}\frac{1}{2}$; so that it does not at first appear why a cold four degrees greater than that should have been produced in the foregoing experiment. The reason is, that at the time the mixture arrived at $-55^{\circ}\frac{1}{2}$, it appeared by the diminution of its fluidity to have contained

contained some undissolved snow, and some more was added to it after that time, which before the first of January dissolved and mixed with the acid; so that the acid in the mixture, at the time it sunk to $-55^{\circ}\frac{1}{4}$, was not quite so much diluted as that which froze on January 1. This is the reverse of what happened in the trial of the nitrous acid in Art. 15. as in that experiment the fluid part, at the time of the greatest cold, was weaker than the whole mixture together; but it must be considered, that *that* mixture contained much congealed acid, as well as undissolved snow, whereas *this* contained only the latter.

25. On January 1, snow was added to the other half of the acid diluted on December 21. The cold produced was much greater than before, namely $-68^{\circ}\frac{1}{4}$; this seems to have proceeded, partly from the air and materials having been 12 degrees colder in this than in the former experiment, and partly from the snow having been added faster, so that the mixture arrived at its greatest degree of cold in 20', whereas it before took up 46'. Another reason is, that the former mixture was made in too small a jar, in consequence of which it was poured into a larger before the experiment was completed, whereby some cold was lost. The quantity of snow used in this experiment was less than in the former, so that the strength of the acid after the experiment was about ,343. The mixture also grew much thicker, and had a degree of elasticity resembling jelly; but whether this was owing only to more snow remaining undissolved, or to any other cause, I cannot tell.

26. Great as the foregoing degree of cold is, Mr. M^c NAB, on February 2, produced one much greater. In hopes of obtaining a greater degree of cold by previously cooling the materials, he cooled about seven ounces of oil of vitriol, whose strength was ,629, that is, rather stronger than the foregoing,

by placing the jar in which it was contained in a freezing mixture of oil of vitriol and snow; the snow intended to be used was also cooled by placing it under the vessel in which the freezing mixture was made. As soon as the acid in the jar was cooled to the temperature of $-57^{\circ}\frac{1}{2}$, a little of the snow was added, on which it immediately began to freeze, and rose to -36° ; but in about 40 minutes, as the jar was still kept in the freezing mixture, it sunk to -48° ; by which time it was grown very thick and gritty, especially at bottom. More of the cooled snow was then added, which in a short time made it sink to $-78^{\circ}\frac{1}{2}$, and at the same time the thickness and tenacity of the mixture diminished; so that by the time it arrived at the greatest degree of cold, very little thickness remained.

It is worth inquiring, what was the reason of the greater degree of cold produced in this than in the preceding experiment? It could not be owing to the materials being colder; for at the time of the second addition of snow, at which time the experiment may be considered to have begun, the acid was not colder than at the beginning of the preceding experiment, and the snow in all probability not much colder. It could not be owing neither to the jar having been kept in the freezing mixture: for though that mixture was three or four degrees colder than the air in the preceding experiment, yet the acid in the jar, before it acquired much addition of cold, would be robbed of its cold faster by the mixture than it would by air of the same temperature as that in the preceding experiment. Neither could it proceed from any difference in the strength of the acid; for what difference there was must have done more hurt than good. The true reason is, that the acid was in a state of congelation: for as the congealed acid united to the snow and became fluid by the union, it is plain, that cold must have been produced both
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by the melting of the snow and by that of the acid; whereas, if the acid had been in a fluid state, cold would have been produced only by the first cause, and consequently a greater degree of cold should be produced in this experiment than in the former. The only inconvenience attending the acid being in a state of congelation is, that in all probability it does not unite to the snow so readily as when in a fluid state; but the difference seems not material, as the cold was produced, and the materials melted, in 5 minutes.

27. The day before, Mr. Mc NAB, by adding snow to some of the same acid in the usual manner, when the cold of the materials was -46° , produced a cold of only -66° .

28. In these four last experiments the acid was reduced, by the addition of the snow, to the strengths of ,325, ,343, ,403, and ,334; and the cold produced in them was before said to be $-55^{\circ}\frac{1}{2}$, $-68^{\circ}\frac{1}{2}$, $-78^{\circ}\frac{1}{2}$, and -66° ; whence we may conclude, that these are nearly the points of aqueous congelation answering to the foregoing strengths; only it appears, from what was said in Art. 24. that the strengths here set down are all of them rather too small.

Though it is certain that oil of vitriol is capable of the spirituous congelation, and though it appears, both from the foregoing experiments and from some made by the Duc d'AYEN* and by M. DE MORVEAU†, that it freezes with a less degree of cold when strong than when much diluted, it is not certain whether it has any point of easiest freezing, like spirit of nitre, or whether the cold required to freeze it does not continually diminish as the strength increases, without limitation; but the latter opinion is the most probable. For the Duc d'AYEN's and

* Diction. de Chym. par MACQUER, 2^e edit.

† Nouv. Mém. de l'Académ. de Dijon, 1782, 1^{er} semestre, p. 68.

M. DE MORVEAU's acids, which, as they were concentrated on purpose, were most likely stronger than Mr. Mc NAB's, froze with a cold less than zero of FAHRENHEIT; whereas the freezing point of Mr. Mc NAB's undiluted acid, whose strength was, 98, was -15° , and that of the diluted acid, whose strength was, 629, was -36° ; and when the acid was more diluted, it was found to bear a much greater cold without freezing. It appears also, both from Art. 21. and from M. DE MORVEAU's experiment, that during the congelation of the oil of vitriol, some separation of its parts takes place, so that the congealed part differs in some respect from the rest, in consequence of which it freezes with a less degree of cold; and as there is reason to think from Art. 21. that these two parts do not differ much in strength, it seems as if the difference between them depended on some less obvious quality, and probably on that, whatever it is, which forms the difference between glacial and common oil of vitriol. The oil of vitriol prepared from green vitriol, has sometimes been obtained in such a state as to remain constantly congealed, except when exposed to a heat considerably greater than that of the atmosphere, whence it acquired its name of *glacial* *. It is not known indeed upon what this property depends, but it is certainly something else than its strength; for oil of vitriol of this kind is always smoking, and the fumes it emits are particularly oppressive and suffocating, though very different from those of the volatile sulphureous acid. On rectification likewise it yields, with the gentlest heat, a peculiar concrete substance, in the form of saline crystals; and after this volatile part has been driven off,

* Mém. de l'Académ. des Sc. 1738, p. 288.

the remainder is no longer smoking, and has lost its glacial quality*.

On the Mixture of Oil of Vitriol and Spirit of Nitre.

29. This mixture is not so fit for producing cold by the addition of snow, as oil of vitriol alone; for the cold obtained did not exceed $-54^{\circ}\frac{1}{2}$, in either of the experiments tried with it. The point of spirituous congelation of this mixture, when diluted with somewhat more than one-tenth of its weight of water, is about -20° , and is much lower when the acid is considerably more diluted: but as the Society will most likely have less curiosity about the disposition to freeze of this mixture than of the simple acids, I shall spare the particulars.

On the Spirit of Wine.

30. The rectified spirits N^o 8. were diluted with snow, in the same manner as the other liquors; but were found not to want any, as the first and only addition of snow produced cold. The quantity added was about $\frac{1}{28}$ of the weight of the spirit.

31. The spirit thus diluted was divided, like the other liquors, into two parts, and each tried separately. The first was at -45° , before the addition of the snow, and was sunk by the process to -56° . The snow, even at the first addition, did not dissolve well, so that the spirit immediately

* CRELL's Neu. Entdeck. in der Chemie, Th. 11. p. 100. Th. 12. p. 241, &c. and Annalen, 1785, St. 5: p. 438, &c.

became full of white spots *, and grew thick by the time it arrived at its greatest degree of cold. After standing some hours, the mixture rose to the temperature of -39° , and was grown clear, but yet was not limpid, but of the consistence of syrup. No cold was produced by adding snow to it in that state, though it appeared that its point of aqueous congelation was at least 6 degrees lower than its temperature at that time †; which seems to shew that spirit of wine has scarce any power of dissolving snow when it wants even 6 degrees of its point of aqueous congelation, and therefore is another instance that snow is dissolved much less readily by spirit of wine than by the nitrous and vitriolic acids.

32. In trying the other part of the diluted spirits, the cold produced was only $-47^{\circ}\frac{1}{2}$, the cold set out with being -37° .

33. It appeared by the diluted spirit of wine N^o 143. which on December 12 froze by the natural cold of the atmosphere, and was treated in the same manner as the diluted spirit of nitre, that when highly rectified spirit of wine, such as N^o 8. is diluted with $1\frac{1}{4}$ its weight of water, its point of aqueous congelation will be at -21° . The congealed part of the spirit was white like diluted milk, and even the decanted part, which was full of thin films of ice, had a milky hue. The fluid part was stronger than the rest, and no increase of cold was produced by adding snow to some of it, both of which are marks of aqueous congelation.

* This was not the case during the above-mentioned dilution of the spirits: but the cold was 16 degrees less in that experiment than in this.

† On account of the dilution which the spirits suffered by the melting of the snow which remained undissolved at the time of the greatest cold, its point of aqueous congelation was no longer so low as -56° ; but it still was not less than $-45\frac{1}{2}$, as in the evening it was found at that temperature, without much congealed matter in it.

Though the foregoing experiments confirm the truth of what I said, in the account of Mr. HUTCHINS's experiments, concerning the cause of the cold produced by mixing snow with different liquors, and intirely clear up the difficulty relating to it which I mentioned in Art. 1. yet several questions may naturally occur; such as, why the cold produced by the oil of vitriol was so much greater than that obtained by the spirit of nitre, notwithstanding that in warmer climates the nitrous acid seems to produce more cold? and why the cold produced by the nitrous acid, notwithstanding its previous dilution, which might naturally be expected to be of service, was not greater than has been obtained by other persons without that precaution? But as this would lead me into disquisitions of considerable length, without my being able to say any thing very satisfactory on the subject, I shall forbear entering into it. I will only observe, that in most of the foregoing experiments, Mr. M^c NAB would probably have produced more cold, if he had added the snow faster. We ought not, however, to regret that he did not, as its effects on the acids would then have been less sensible.

The natural cold, when these experiments were made, is remarkable; as there were at least nine mornings in which the cold was not less than that of freezing mercury; four in which it was at least eight degrees below that point, or -47° ; and one in which it was -50° . Whereas out of nine winters, during which Mr. HUTCHINS observed the thermometer at Albany Fort, there were only twelve days in which the cold was equal to that of freezing mercury, and the greatest cold seems to have been -45° . I cannot learn whether the last winter was more severe than usual at Hudson's Bay; or whether Henley-House is a colder situation than Albany, which
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may perhaps be the case; for though it is only 130 miles distant from it, yet it stands inland, and to the W. or S.W. of it, which is the quarter from which the coldest winds blow.

Mr. M^c NAB's original account of the experiments which furnished the materials of this Paper, having been thought too long to be printed in detail, is deposited in the Archives of the Society.

END OF PART I. OF VOL. LXXVI.

PHILOSOPHICAL
TRANSACTIONS.

XIV. *New Experiments upon Heat. By Colonel Sir Benjamin Thompson, Knt. F. R. S. In a Letter to Sir Joseph Banks, Bart. P. R. S.*

Read March 9, 1786.

DEAR SIR,

I HAVE at length begun the course of experiments upon heat which I have so long had in contemplation; and I have already made a discovery, which, if not new to you, is perfectly so to me, and which I think may lead to a further knowledge respecting the nature of heat.

VOL. LXXVI.

O o

Examining

Examining the conducting power of air, and of various other fluid and solid bodies, with regard to heat, I was led to examine the conducting power of the *Torricellian vacuum*. From the striking analogy between the electric fluid and heat respecting their conductors and non-conductors (having found that bodies, in general, which are conductors of the electric fluid, are likewise good conductors of heat, and, on the contrary, that electric bodies, or such as are bad conductors of the electric fluid, are likewise bad conductors of heat), I was led to imagine that the Torricellian vacuum, which is known to afford so ready a passage to the electric fluid, would also have afforded a ready passage to heat.

The common experiments of heating and cooling bodies under the receiver of an air-pump I concluded inadequate to determining this question; ~~not only~~ on account of the impossibility of making a perfect void of air by means of the pump; but also on account of the moist vapour, which exhaling from the wet leather and the oil used in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of heat: I had recourse therefore to other contrivances.

I took a thermometer, unfilled, the diameter of whose bulb (which was globular) was just half an inch, Paris measure, and fixed it in the center of a hollow glass ball of the diameter of $1\frac{1}{2}$ Paris inch, in such a manner, that the short neck or opening of the ball being soldered fast to the tube of the thermometer $7\frac{1}{2}$ lines above its bulb, the bulb of the thermometer remained fixed in the center of the ball, and consequently was cut off from all communication with the external air. In the bottom of the glass ball was fixed a small hollow tube or point, which projecting outwards was soldered to

to the end of a common barometer tube about 32 inches in length, and by means of this opening the space between the internal surface of the glass ball and the bulb of the thermometer was filled with hot mercury, which had been previously freed of air and moisture by boiling. The ball, and also the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The instrument now became a barometer, and the mercury descending from the ball (which was now uppermost) left the space surrounding the bulb of the thermometer free of air. The mercury having totally quitted the glass ball, and having sunk in the tube to the height of 28 inches (being the height of the mercury in the common barometer at that time), with a lamp and a blow-pipe I melted the tube together, or sealed it hermetically, about three-quarters of an inch below the ball, and cutting it at this place with a fine file, I separated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, I now possessed a thermometer whose bulb was confined in the center of a *Torricellian vacuum*, and which served at the same time as the body to be heated, and as the instrument for measuring the heat communicated.

Experiment N^o 1.

With this instrument (see fig. 1. Tab. VI.) I made the following experiment. Having plunged it into a vessel filled with water, warm to the 18th degree of REAUMUR's scale, and suffered it to remain there till it had acquired the temperature of the water, that is

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to say, till the mercury in the inclosed thermometer stood at 18° , I took it out of this vessel and plunged it suddenly into a vessel of boiling water, and holding it in the water (which was kept constantly boiling) by the end of the tube, in such a manner that the glass ball, in the center of which was the bulb of the thermometer, was just submerged, I observed the number of degrees to which the mercury in the thermometer had arisen at different periods of time, counted from the moment of its immersion. Thus, after it had remained in the boiling water 1 min. 30 sec. I found the mercury had risen from 18° to 27° . After 4 minutes had elapsed, it had risen to $44^{\circ} \frac{2}{3}$; and at the end of 5 minutes it had risen to $48^{\circ} \frac{1}{2}$.

Experiment N^o 2.

Taking it now out of the boiling water I suffered it to cool gradually in the air, and after it had acquired the temperature of the atmosphere, which was that of 15° R. (the weather being perfectly fine), I broke off a little piece from the point of the small tube which remained at the bottom of the glass ball, where it had been hermetically sealed, and of course the atmospheric air rushed immediately into the ball. The ball surrounding the bulb of the thermometer being now filled with air (instead of being emptied of air, as it was in the before-mentioned experiment), I re-sealed the end of the small tube at the bottom of the glass ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the instrument so prepared I repeated the experiment before-mentioned; that is to say, I put it into water warmed to 18° , and when it had acquired the temperature

perature of the water, I plunged it into boiling water, and observed the times of the ascent of the mercury in the thermometer. They were as follows :

	Time elapsed.	Heat acquired.
Heat at the moment of being plunged into the boiling water,		18° R.
	M. S.	°
After having remained in the boiling water	0 45	27
	1 0	34 $\frac{1}{8}$
	2 10	44 $\frac{9}{16}$
	2 40	48 $\frac{3}{8}$
	4 0	56 $\frac{3}{8}$
	5 0	60 $\frac{9}{16}$

From the result of these experiments it appears evidently, that the Torricellian vacuum, which affords so ready a passage to the electric fluid, so far from being a good conductor of heat, is a much worse one than common air, which of itself is reckoned among the worst: for in the last experiment, when the bulb of the thermometer was surrounded with air, and the instrument was plunged into boiling water, the mercury rose from 18° to 27° in 45 seconds; but in the former experiment, when it was surrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute 30 seconds = 90 seconds, to acquire that degree of heat. In the vacuum it required 5 minutes to rise to 48 $\frac{3}{8}$; but in air it rose to that height in 2 minutes 40 seconds; and the proportion of the times in the other observations is nearly the same, as will appear by the following table.

The

The bulb of the thermometer placed in the

air.

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After remaining in it	1 30	27	0 45	27
	—	—	1 0	30 $\frac{4}{8}$
	4 0	44 $\frac{9}{8}$	2 10	44 $\frac{9}{8}$
	5 0	48 $\frac{2}{8}$	2 40	48 $\frac{2}{8}$
	—	—	4 0	56 $\frac{2}{8}$
	—	—	5 0	60 $\frac{2}{8}$

These experiments were made at Manheim, upon the first day of July last, in the presence of Professor HEMMER, of the Electoral Academy of Sciences of Manheim, and CHARLES ARTARIA, Meteorological Instrument-maker to the Academy, by whom I was assisted.

Finding the construction of the instrument made use of in these experiments attended with much trouble and risque, on account of the difficulty of soldering the glass ball to the tube of the thermometer without at the same time either closing up, or otherwise injuring, the bore of the tube, I had recourse to another contrivance much more commodious, and much easier in the execution.

At the end of a glass tube or cylinder ten or eleven inches in length, and near three-quarters of an inch in diameter internally, I caused a hollow globe to be blown $1\frac{1}{2}$ inch in diameter,

meter, with an opening in the bottom of it corresponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or short tube, about an inch or three-quarters of an inch in length. Having a thermometer prepared, whose bulb was just half an inch in diameter, and whose freezing point fell at about $2\frac{1}{4}$ inches above its bulb, I graduated its tube according to REAUMUR's scale, beginning at 0° , and marking that point, and also every tenth degree above it to 80° , with threads of fine silk bound round it, which being moistened with lac varnish adhered firmly to the tube. This thermometer I introduced into the glass cylinder and globe just described, by the opening in the bottom of the globe, having first choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its sides inwards towards its axis, leaving only an opening sufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in such a manner that the center of its bulb coincided with the center of the globe, I marked a place in the cylinder, about three-quarters of an inch above the 80th degree or boiling point upon the tube of the inclosed thermometer, and taking out the thermometer, I choaked the cylinder again in this place. Introducing now the thermometer for the last time, I closed the opening at the bottom of the globe at the lamp, taking care, before I brought it to the fire, to turn the cylinder upside down, and to let the bulb of the thermometer fall into the cylinder till it rested upon the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now closed, and the bulb of the thermometer being suffered to return into the globe, the end of the cylinder was cut off to within

within about half an inch of the upper choak. This being done, it is plain, that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as possible in the center of the globe, and observing and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upside down, and suffering the bulb of the thermometer to enter the cylinder, and rest upon the first or lower choak (by which means the end of the tube of the thermometer came further out of the cylinder), the end of the tube was cut off at the mark just mentioned (having first taken care to melt the internal cavity or bore of the tube together at that place), and a small solid ball of glass, a little larger than the internal diameter or opening of the choak, was soldered to the end of the tube, forming a little button or knob, which resting upon the upper choak of the cylinder, served to suspend the thermometer in such a manner that the center of its bulb coincided with the center of the globe in which it was shut up. The end of the cylinder above the upper choak being now heated and drawn out to a point, or rather being formed into the figure of the frustum of a hollow cone, the end of it was soldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically sealed, the barometrical tube was detached from it with a file, and the thermometer was left completely shut up in a Torricellian vacuum, the center of the bulb of the thermometer being confined in the center of the glass globe, without touching it in any part, by means of the two choaks in the cylinder, and the button upon the end of the tube.

Of these instruments I provided myself with two, as nearly as possible of the same dimensions; the one, which I shall call N° 1. being voided of air, in the manner above described; the other, N° 2. being filled with air, and hermetically sealed.

With these two instruments (see fig. 2.) I made the following experiments upon the 11th of July last, at Manheim, between the hours of ten and twelve, the weather being very fine and clear, the mercury in the barometer standing at 27 inches 11 lines, REAUMUR's thermometer at 15°, and the quill hygrometer of the Academy of Manheim at 47°.

Experiments N° 3, 4, 5 and 6.

Putting both the instruments into melting ice, I let them remain there till the mercury in the inclosed thermometers rested at the point 0°, that is to say, till they had acquired exactly the temperature of freezing water or melting ice; and then taking them out of the ice I plunged them suddenly into a large vessel of boiling water, and observed the time required for the mercury to rise in the thermometers from ten degrees to ten degrees, from 0° to 80°, taking care to keep the water constantly boiling during the whole of this time, and taking care also to keep the instruments immersed to the same depth, that is to say, just so deep that the point 0° of the inclosed thermometer was even with the surface of the water.

These experiments I repeated twice, with the utmost care; and the following table gives the result of them.

power of air to that of the Torricellian vacuum, under the circumstances described, is as $7\frac{3}{4}$ to $10\frac{484}{60}$ inversely, or as 1000 to 702 nearly ; for the quantities of heat communicated being equal,

equal, the intensity of the communication is as the times inversely.

In these experiments the heat passed through the surrounding medium *into* the bulb of the thermometer: in order to reverse the experiment, and make the heat pass *out of* the thermometer, I put the instruments into boiling water, and let them remain therein till they had acquired the temperature of the water, that is to say, till the mercury in the inclosed thermometers stood at 80°; and then, taking them out of the boiling water, I plunged them suddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I observed the times employed in cooling as follows.

Thermometer N° 1. Surrounded by a Torricellian vacuum. <i>Taken out of boiling water, and plunged into freezing water.</i>			Thermometer N° 2. Surrounded by air. <i>Taken out of boiling water, and plunged into freezing water.</i>		
Time elapsed.		Heat lost.	Time elapsed.		Heat lost.
Exp. N° 7.	Exp. N° 8.		Exp. N° 9.	Exp. N° 10.	
		80°			80°
M. S.	M. S.		M. S.	M. S.	
1 2	0 54	70°	0 33	0 33	70°
0 58	1 2	60°	0 39	0 34	60°
1 17	1 18	50°	0 44	0 44	50°
1 46	1 37	40°	0 55	0 55	40°
2 5	2 16	30°	1 17	1 18	30°
3 14	3 10	20°	1 57	1 57	20°
5 42	5 59	10°	3 44	3 40	10°
Not observed.	Not observed.	0°	40 10	Not observed.	0°
Total time of cooling from 80° to 10°.			Total time of cooling from 80° to 10°.		
M. S.			M. S.		
In Exp. N° 7. = 16 4			In Exp. N° 9. = 9 49		
In Exp. N° 8. = 16 16			In Exp. N° 10. = 9 41		
Medium = 16 10			Medium = 9 45		

80° to 70°, being at that time busied in suspending the instruments.

As it might possibly be objected to the conclusions drawn from these experiments that, notwithstanding all the care that was taken in the constructing of the two instruments made use of that they should be perfectly alike, yet they might in reality be so far different, either in shape or size, as to occasion a very sensible error in the result of the experiments; to remove these doubts I made the following experiments.

In the morning towards eleven o'clock, the weather being remarkably fine, the mercury in the barometer standing at 27 inches 11 lines, REAUMUR's thermometer at 15°, and the hygrometer at 47°, I repeated the experiment N° 3. (of heating the thermometer N° 1. in boiling water, &c.) and immediately afterwards opening the cylinder containing the thermometer at its upper end, where it had been sealed, and letting the air into it, I re-sealed it hermetically, and repeated the experiment again with the same instrument, the thermometer being now surrounded with air, like the thermometer N° 2.

The result of these experiments, which may be seen in the following table, shews evidently, that the error arising from the difference of the shapes or dimensions of the two instruments in question was inconsiderable, if not totally imperceptible.

(Exp.

Torricellian vacuum as $7\frac{1}{8}\%$ to $11\frac{1}{8}\%$ inversely, or as 1000 to 602; which differs but very little from the result of all the foregoing experiments.

Notwithstanding that it appeared, from the result of these last experiments, that any difference there might possibly have been in the proportions or dimensions of the instruments N° 1. and N° 2. could hardly have produced any sensible error
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in the result of the experiments in question; I was willing, however, to see how far any considerable alterations of size in the instrument would affect the experiment: I therefore provided myself with another instrument, which I shall call *Thermometer* N^o 3. different from those already described in size, and a little different in its construction.

The bulb of the thermometer was of the same form and size as in the instruments N^o 1. and N^o 2. that is to say, it was globular, and half an inch in diameter; but the glass globe, in the center of which it was confined, was much larger, being 3 inches $7\frac{1}{2}$ lines in diameter; and the bore of the tube of the thermometer was much finer, and consequently its length, and the divisions of its scale, were greater. The divisions were marked upon the tube with threads of silk of different colours at every tenth degree, from 0° to 80° , as in the before-mentioned instruments. The tube or cylinder belonging to the glass globe was 8 lines in diameter, a little longer than the tube of the thermometer, and perfectly cylindrical from its upper end to its junction with the globe, being without any choak; the thermometer being confined in the center of the globe by a different contrivance, which was as follows. To the opening of the cylinder was fitted a stopple of dry wood, covered with a coat of hard varnish, through the center or axis of which passed the end of the tube of the thermometer: this confined the tube in the axis of the cylinder at its upper end. To confine it at its lower end, there was fitted to it a small steel spring, a little below the point 0° ; which, being confined round the tube of the thermometer, had three elastic points projecting outwards, which pressing against the inside of the cylinder, confined the thermometer in its place. The total length of this instrument, from the bottom of the globe

to

to the upper end of the cylinder, was 18 inches, and the freezing point upon the thermometer fell about 3 inches above the bulb; consequently it lay about $1\frac{1}{2}$ inch above the junction of the cylinder with the globe, when the thermometer was confined in its place, the center of its bulb coinciding with the center of the globe. Through the stopple which closed the end of the cylinder passed two small glass tubes, about a line in diameter, which being about a line longer than the stopple were stopped up occasionally with small stopples fitted to their bores. These tubes (which were fitted exactly in the holes bored in the great stopple of the cylinder to receive them, and fixed in their places with cement) served to convey air, or any other fluid, into the glass ball, without being under a necessity of removing the stopple closing the end of the cylinder; which, in order to prevent the position of the thermometer from being easily deranged, was cemented in its place.

I have been the more particular in the description of these instruments, as I conceive it absolutely necessary to have a perfect idea of them in order to judge of the experiments made with them.

With the instrument last described (which I have called *Thermometer N° 3.*) I made the following experiment. It was upon the 18th of July last, in the afternoon, the weather variable, alternate clouds and sun-shine; wind strong at S.E. with now and then a sprinkling of rain; barometer at 27 inches $10\frac{1}{2}$ lines, thermometer at $18^{\circ}\frac{1}{4}$, and hygrometer variable from 44° to extreme moisture.

In order to compare the result of the experiment made with this thermometer with those made with the thermometer N° 2. I have, in the following table, placed these experiments by the side of each other.

(Exp.

(Exp. N ^o 15.) Thermometer N ^o 3.		(Exp. N ^o 4. and N ^o 5.) Thermometer N ^o 2.			
Its bulb half an inch in diameter, shut up in the center of a glass tube, 3 inches $7\frac{1}{2}$ lines in diameter, and surrounded by air.		Its bulb half an inch in diameter, shut up in the center of a glass globe, $1\frac{1}{2}$ inch in diameter, and surrounded by air.			
Taken out of freezing water, and plunged into boiling water.		Taken out of freezing water, and plunged into boiling water.			
Time elapsed.		Time elapsed.			Heat acquired.
Heat acquired.		Exp. N ^o 4.	Exp. N ^o 5.	Medium.	Heat acquired.
O ^o		O ^o			O ^o
M. S.		M. S.	M. S.	M. S.	
0 33	10	0 30	0 30	0 30	10
0 38	20	0 35	0 37	0 36	20
0 54	30	0 41	0 41	0 41	30
0 51	40	0 49	0 53	0 51	40
1 7	50	1 1	0 59	1 0	50
1 28	60	1 24	1 20	1 22	60
2 28	70	2 45	2 25	2 35	70
9 0	80	9 10	9 38	9 24	80
16 59 = total time of heating from 0 ^o to 80 ^o .		16 55	17 3	16 59 = total	
Time from 0 ^o to 70 ^o = 7' 59".		Time of heating from 0 ^o to 80 ^o .			
		Time from 0 ^o to 70 ^o = 7' 35"			

If the agreement of these experiments with the thermometers N^o 2. and N^o 3. surprised me, I was not less surprised with their disagreement in the experiment which follows.

Experiment N^o 16.

Taking the thermometer N^o 3. out of the boiling water, I immediately suspended it in the middle of a large room, where the air, which was quiet, had the temperature of 18^o F. and observed the times of cooling as follows:

Time elapsed.		Heat lost.
<hr/>		80°
M.	S.	°
1	55	70°
0	12	60°
0	33	50°
2	15	40°
4	0	30°
<hr/>		

9' 55" = total time of cooling from 80° to 30°.

Time from 70° to 30° = 8' 0"; but in the experiment N° 12. with the thermometer N° 2. the time employed in cooling from 70° to 30° was only 6' 11". In this experiment, with the thermometer N° 3. the time employed in cooling from 60° to 50° was 7' 48"; but in the above-mentioned experiment, with the thermometer N° 2. it was only 5' 20". It is true, the air of the room was somewhat cooler when the former experiment was made than when this latter was made with the thermometer N° 3.; but this difference of temperature, which was only $2^{\circ}\frac{1}{4}$ (in the former case the thermometer in the room standing at 16°, and in the latter at 18 $^{\circ}\frac{1}{4}$) certainly could not have occasioned the whole of the apparent difference in the results of the experiments.

Does air receive heat more readily than it parts with it? This is a question highly deserving of further investigation, and I shall not fail to give it a full examination in the course of my projected inquiries; but leaving it for the present, I shall proceed to give an account of the experiments which I have already made*.

It

* Conceiving it to be a step of considerable importance towards coming at a further

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gasses, the thermometer

further knowledge of the nature of heat, to ascertain, by indisputable evidence, its passage through the Torricellian vacuum, and to determine, with as much precision as possible, the law of its motions in that medium; and being apprehensive that doubts might arise with respect to the experiments before described, on account of the contact of the tubes of the inclosed thermometers in the instruments made use of with the containing glass globes, or rather with their cylinders; by which means it might be suspected, that a certain quantity, if not all the heat acquired, might possibly be communicated: to put this matter beyond all doubt, I made the following experiment.

In the middle of a glass body, of a pear-like form, about 8 inches long, and $2\frac{1}{2}$ inches in its greatest diameter, I suspended a small mercurial thermometer, $5\frac{1}{2}$ inches long, by a fine thread of silk, in such a manner that neither the bulb of the thermometer, nor its tube, touched the containing glass body in any part. The tube of the thermometer was graduated, and marked with fine threads of silk of different colours bound round it, as in the thermometers belonging to the other instruments, already described; and the thermometer was suspended in its place by means of a small steel spring, to which the end of the thread of silk which held the thermometer being attached, it (the spring) was forced into a small globular protuberance or cavity, blown in the upper extremity of the glass body, about half an inch in diameter, where the spring remaining, the thermometer necessarily remained suspended in the axis of the glass body. There was an opening at the bottom of the glass body, through which the thermometer was introduced; and a barometrical tube being soldered to this opening, the inside of the glass body was voided of air by means of mercury; and this opening being afterwards sealed hermetically, and the barometrical tube being taken away, the thermometer was left suspended in a Torricellian vacuum.

In this instrument, as the inclosed thermometer did not touch the containing glass body in any part, on the contrary, being distant from its internal surface an inch or more in every part, it is clear, that whatever heat passed into or out of the thermometer must have passed through the surrounding Torricellian vacuum: for it cannot be supposed, that the fine thread of silk, by which the thermometer was suspended, was capable of conducting any heat at all, or at least any sensible quantity. I therefore flattered myself with hopes of being able, with the

assistance

thermometer N^o 3. was constructed with a view to those experiments; and having now provided myself with a stock of those different kinds of airs, I began with *fixed air*, with which, by

assistance of this instrument, to determine positively with regard to the passage of heat in the Torricellian vacuum: and this, I think, I have done, notwithstanding that an unfortunate accident put it out of my power to pursue the experiments so far as I intended.

This instrument being fitted to a small stand or foot of wood, in such a manner that the glass body remained in a perpendicular situation, I placed it in my room, by the side of another inclosed thermometer (N^o 2.), which was surrounded by air, and observed the effect of the variation of heat in the atmosphere. I soon discovered, by the motion of the mercury in the inclosed thermometer, that the heat passed through the Torricellian vacuum; but it appeared plainly from the sluggishness or great insensibility of the thermometer, that the heat passed with much greater difficulty in this medium than in common air. I now plunged both the thermometers into a bucket of cold water; and I observed that the mercury in the thermometer surrounded by air descended much faster than that in the thermometer surrounded by the Torricellian vacuum. I took them out of the cold water, and plunged them into a vessel of hot water (having no convenience at hand to repeat the experiment in form with the freezing and with the boiling water); and the thermometer surrounded by the Torricellian vacuum appeared still to be much more insensible or sluggish than that surrounded by air.

These trials were quite sufficient to convince me of the passage of heat in the Torricellian vacuum, and also of the greater difficulty of its passage in that medium than in common air; but, not satisfied to rest my inquiries here, I took the first opportunity that offered, and set myself to repeat the experiments which I had before made with the instruments N^o 1. and N^o 2. I plunged this instrument into freezing water, where I let it remain till the mercury in the inclosed thermometer had descended to 0°; when, taking it out of the freezing water, I plunged it suddenly into a vessel of boiling water, and prepared myself to observe the ascent of the mercury in the inclosed thermometer as in the foregoing experiments; but unfortunately the moment the end of the glass body touched the boiling water, it cracked with the heat at the point where it had been hermetically sealed, and the water rushing into the body, spoiled the experiment: and I have not since had an opportunity of providing myself with another instrument to repeat it.

means

means of water, I filled the globe and cylinder containing the thermometer; and stopping up the two holes in the great stopple closing the end of the cylinder, I exposed the instrument in freezing water till the mercury in the inclosed thermometer had descended to 0° ; when, taking it out of the freezing water, I plunged it into a large vessel of boiling water, and prepared myself to observe the times of heating, as in the former cases; but an accident happened, which suddenly put a stop to the experiment. Immediately upon plunging the instrument into the boiling water, the mercury began to rise in the thermometer with such uncommon celerity, that it had passed the first division upon the tube (which marked the 10th degree, according to REAUMUR'S scale) before I was aware of its being yet in motion; and having thus missed the opportunity of observing the time elapsed when the mercury arrived at that point, I was preparing to observe its passage of the next, when all of a sudden the stopple closing the end of the cylinder was blown up the chimney with a great explosion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and destroyed in its fall.

This unfortunate experiment, though it put a stop for the time to the inquiries proposed, opened the way to other researches not less interesting. Suspecting that the explosion was occasioned by the rarefaction of the water which remained attached to the inside of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity, with which the mercury rose in the thermometer, was principally owing to the same cause; I was led to examine the conducting power of *mist air*, or air saturated with water.

For

For this experiment I provided myself with a new thermometer N° 4. the bulb of which, being of the same form as those already described (*viz.* globular) was also of the same size, or half an inch in diameter. To receive this thermometer a glass cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe 1½ inch in diameter. In the center of this globe the bulb of the thermometer was confined, by means of the stopple which closed the end of the cylinder; which stopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its center or axis, and confined the thermometer in its place, without the assistance of any other apparatus. Through this stopple two other small holes were bored, and lined with thin glass tubes, as in the thermometer N° 3. opening a passage into the cylinder, which holes were occasionally stopped up with some stopples of cork; but to prevent accidents, such as I had before experienced from an explosion, great care was taken not to press these stopples into their places with any considerable force, that they might the more easily be blown out by any considerable effort of the confined air.

Though in this instrument the thermometer was not altogether so steady in its place as in the thermometers N° 1. N° 2. and N° 3. the elasticity of the tube, and the weight of the mercury in the bulb of the thermometer, occasioning a small vibration or trembling of the thermometer upon any sudden motion or jar; yet I preferred this method to the others, on account of the lower part of this thermometer being entirely free, or suspended in such a manner as not to touch, or have any communication with, the lower part of the cylinder or the globe: for though the quantity of heat received by the tube of the thermometer at its contact with the cylinder at its choaks,

choaks, in the instruments N^o 1. and N^o 2. or with the branches of the steel spring in N^o 3. and from thence communicated to the bulb, must have been exceedingly small; yet I was desirous to prevent even that, and every other possible error or inaccuracy, however small, that might arise.

Does humidity augment the conducting power of air?

To determine this question I made the following experiments, the weather being clear and fine, the mercury in the barometer standing at 27 inches 8 lines, the thermometer at 19°, and the hygrometer at 44°.

(Exp. N ^o 17.) <i>Thermometer N^o 4.</i>			(Exp. N ^o 18.) <i>The same thermometer (N^o 4.)</i>		
Surrounded by air dry to the 44th degree of the quill hygrometer of the Manheim Academy.			Surrounded by air rendered as moist as possible by wetting the inside of the cylinder and globe with water.		
<i>Taken out of freezing water, and plunged into boiling water.</i>			<i>Taken out of freezing water, and plunged into boiling water.</i>		
Time elapsed.		Heat acquired.	Time elapsed.		Heat acquired.
		80°			0°
M.	S.		M.	S.	
0	34	10°	0	6	10°
0	39	20°	0	4	20°
0	44	30°	0	5	30°
0	51	40°	0	9	40°
1	6	50°	0	18	50°
1	35	60°	0	26	60°
2	40	70°	0	43	70°
not observed.		80°	7	45	80°
8 9 = total time of heating from 0° to 70°.			1 51 = total time of heating from 0° to 70°.		

From

From these experiments, it appears, that the conducting power of air is very much increased by humidity. To see if the same result would obtain when the experiment was reversed, I now took the thermometer with the *moist air* out of the boiling water, and plunged it into freezing water; and moving it about continually from place to place in the freezing water, I observed the times of cooling, as set down in the following table. N. B. To compare the result of this experiment with those made with *dry air*, I have placed on one side in the following table the experiment in question, and on the other side the experiment N^o 19. made with the thermo-

small, yet the difference of the times taken up by the first twenty or thirty degrees from the boiling point is very remarkable, and shows with how much greater facility heat passes in moist air than in dry air. Even the slowness with which the mercury in the thermometer N^o 4. descended in this experiment from the 30th to the 20th, and from the 20th to the 10th degree, I attribute in some measure to the great conducting power of the moist air with which it was surrounded; for the cylinder containing the thermometer and the moist air, being not wholly submerged in the freezing water, that part of it which remained out of the water was necessarily surrounded by the air of the atmosphere; which being much warmer than the water, communicated of its heat to the glass; which, passing from thence into the contained moist air as soon as that air became colder than the external air, was, through that medium, communicated to the bulb of the inclosed thermometer, which prevented its cooling so fast as it would otherwise have done. But when the weather becomes cold, I propose to repeat this experiment with variations, in such a manner as to put the matter beyond all doubt. In the mean time I cannot help observing, with what infinite wisdom and goodness Divine Providence appears to have guarded us against the evil effects of excessive heat and cold in the atmosphere; for if it were possible for the air to be equally damp during the severe cold of the winter months as it sometimes is in summer, its conducting power, and consequently its apparent coldness, when applied to our bodies, would be so much increased, by such an additional degree of moisture, that it would become quite intolerable; but, happily for us, its power to hold water in solution is diminished, and with it its power to rob us of our animal heat, in proportion as its

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coldness is increased. Every body knows how very disagreeable a very moderate degree of cold is when the air is very damp; and from hence it appears, why the thermometer is not always a just measure of the apparent or sensible heat of the atmosphere. If colds or catarrhs are occasioned by our bodies being robbed of our animal heat, the reason is plain why those disorders prevail most during the cold autumnal rains, and upon the breaking up of the frost in the spring. It is likewise plain from whence it is that sleeping in damp beds, and inhabiting damp houses, is so very dangerous; and why the evening air is so pernicious in summer and in autumn, and why it is not so during the hard frosts of winter. It has puzzled many very able philosophers and physicians to account for the manner in which the extraordinary degree or rather *quantity* of heat is generated which an animal body is supposed to lose, when exposed to the cold of winter, above what it communicates to the surrounding atmosphere in warm summer weather; but is it not more than probable, that the difference of the quantities of heat, actually lost or communicated, is infinitely less than what they have imagined? These inquiries are certainly very interesting; and they are undoubtedly within the reach of well contrived and well conducted experiments. But taking my leave for the present of this curious subject of investigation, I hasten to the sequel of my experiments.

Finding so great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of density. For this experiment I prepared the thermometer N^o 4. by stopping up one of the small glass tubes passing through the stopple, and opening a passage into the cylinder, and by fitting a valve to the external oriture of the other.

other. The instrument, thus prepared, being put under the receiver of an air-pump, the air passed freely out of the globe and cylinder upon working the machine, but the valve above described prevented its return upon letting air into the receiver. The gage of the air-pump showed the degree of rarity of the air under the receiver, and consequently of that filling the globe and cylinder, and immediately surrounding the thermometer.

With this instrument, the weather being clear and fine, the mercury in the barometer standing at 27 inches 9 lines, the thermometer at 15° , and the hygrometer at 47° , I made the following experiments.

(Exp. N ^o 20.) Thermometer N ^o 4. Surrounded by common air, barometer standing at 27 inches 9 lines. <i>Taken out of freezing wa- ter, and plunged into boiling water.</i>		(Exp. N ^o 21.) Thermometer N ^o 4. Surrounded by air rare- fied by pumping till the barometer-gage stood at 6 inches $11\frac{1}{2}$ lines. <i>Taken out of freezing wa- ter, and plunged into boiling water.</i>		(Exp. N ^o 22.) Thermometer N ^o 4. Surrounded by air rare- fied by pumping till the barometer-gage stood at 1 inch 2 lines. <i>Taken out of freezing wa- ter, and plunged into boiling water.</i>	
Time elapsed.	Heat acquired. °	Time elapsed.	Heat acquired. °	Time elapsed.	Heat acquired. °
M. S.	°	M. S.	°	M. S.	°
0 31	10	0 31	10	0 29	10
0 40	20	0 38	20	0 36	20
0 41	30	0 44	30	0 49	30
0 47	40	0 51	40	1 1	40
1 4	50	1 7	50	1 1	50
1 25	60	1 19	60	1 24	60
2 28	70	2 27	70	2 31	70
10 17	80	10 21	80	not observed.	80
7 36 = total time of heating from 0° to 70° .		7 37 = total time of heating from 0° to 70° .		7 51 = total time of heating from 0° to 70° .	

The result of these experiments, I confess, surprised me not a little; but the discovery of truth being the sole object of my inquiries (having no favourite theory to defend) it brings no disappointment along with it, under whatever unexpected shape it may appear. I hope that further experiments may lead to the discovery of the cause why there is so little difference in the conducting powers of air of such very different degrees of rarity, while there is so great a difference in the conducting powers of air, and of the Torricellian vacuum. At present, I shall not venture any conjectures upon the subject; but in the mean time I dare to assert, that the experiments I have made may be depended on.

The time of my stay at Mannheim being expired (having had the honour to attend thither his most Serene Highness the Elector Palatine Duke of Bavaria, my most Gracious Master, in his late journey), I was prevented from pursuing these inquiries further at that time; but I shall not fail to recommence them the first leisure time I can find, which I fancy will be about the beginning of the month of November. In the mean time, to enable myself to pursue them with effect, I am sparing neither labour nor expence to provide a complete apparatus necessary for my purpose; and his Electoral Highness has been graciously pleased to order M. ARTARIA (who is in his service) to come to Munich to assist me. With such a Patron as his most Serene Highness, and with such an assistant as ARTARIA, I shall go on in my pursuits with cheerfulness. Would to God that my labours might be as useful to others as they will be pleasant to me!

I shall conclude this letter with a short account of some experiments I have made to determine the conducting powers

of water and of mercury; and with a table, showing at one view the conducting powers of all the different mediums which I have examined.

Having filled the glass globe inclosing the bulb of the thermometer N^o. 4. first with water, and then with mercury, I made the following experiments, to ascertain the conducting powers of those two fluids.

(Exp. N ^o 23.) Thermometer N ^o 4. Surrounded by water. <i>Taken out of freezing water, and plunged into boiling water.</i>		(Exp. N ^o 24, 25, and 26.) Thermometer N ^o 4. Surrounded by mercury. <i>Taken out of freezing water, and plunged into boiling water.</i>			
Time elapsed.	Heat acquired.	Time elapsed.			Heat acquired.
M. S.	°	Exp. N ^o 24.	Exp. N ^o 25.	Exp. N ^o 26.	°
0 19	10	0 5	0 5	0 5	10
0 8	20	0 4	0 4	0 5	20
0 9	30	0 4	0 2	0 4	30
0 11	40	0 4	0 5	0 5	40
0 15	50	0 4	0 4	0 7	50
0 21	60	0 7	0 4	0 8	60
0 34	70	0 15	0 9	0 14	70
2 13	80	Not observed.	0 58	Not observed.	80
1 57 = total time of heating from 0° to 70°.		0 41	0 31	0 48 = total times of heating from 0° to 70°.	

The total times of heating from 0° to 70° in the three experiments with mercury being 41 seconds, 31 seconds, and 48 seconds, the mean of these times is 36½ seconds; and as in the experiment with water the time employed in acquiring the same degree of heat was 1' 57" = 117 seconds, it appears from

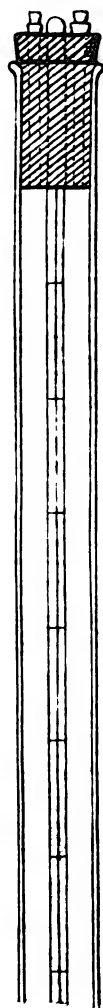
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A Table



Fig. 4.



A Table of the conducting Powers of the under-mentioned Mediums, as determined by the foregoing experiments.

Thermom. N° 1.	Thermometer N° 4.						
Taken out of freezing water, and plunged into boiling water.							
Time elapsed.							
Toricellian Va- cuum (Exp. N° 3. 4. and 13.)	Common air, density = 1, (Exp. N° 20.)	Rarefied air, density = $\frac{1}{4}$ (Exp. N° 21.)	Rarefied air, density = $\frac{1}{12}$ (Exp. N° 22.)	Moist air (Exp. N° 18.)	Water (Exp. N° 23.)	Mercury (Exp. N° 24, 25, and 26.)	Heat acquired.
— M. S.	— M. S.	— M. S.	— M. S.	— M. S.	— M. S.	— M. S.	°
0 52	0 31	0 31	0 29	0 6	0 19	0 5	10
0 58	0 40	0 38	0 36	0 4	0 8	0 3 $\frac{1}{2}$	20
1 3	0 41	0 44	0 49	0 5	0 9	0 2 $\frac{1}{2}$	30
1 18	0 47	0 51	1 1	0 9	0 11	0 4 $\frac{1}{2}$	40
1 25	1 4	1 7	1 1	0 18	0 15	0 5	50
1 58	1 25	1 19	1 24	0 26	0 21	0 6 $\frac{1}{2}$	60
3 19	2 28	2 27	2 31	0 43	0 34	0 12 $\frac{1}{2}$	70
11 57	10 17	10 21	—	7 45	2 13	0 58	80
10 53	7 36	7 37	7 51	1 51	1 57	0 36 $\frac{1}{2}$	= to-
the times of heating from 0° to 70°.							

In determining the relative conducting powers of these mediums, I have compared the times of the heating of the thermometers from 0° to 70° instead of taking the whole times from 0° to 80°, on account of the small variation in the heat of the boiling water arising from the variation of the weight of the atmosphere, and also on account of the very slow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the precise moment when the mercury arrives at the 80th degree.

Taking

long neglected field of experimental inquiry. For my own part, I am determined not to quit it.

In the future prosecution of these inquiries, I do not mean to confine myself solely to the determining of the conducting powers of fluids; on the contrary, solids, and particularly such bodies as are made use of for cloathing, will be principal subjects of my future experiments. I have indeed already begun these researches, and have made some progress in them; but I forbear to anticipate a matter which I propose for the subject of a future communication.



XV. History and Dissection of an extraordinary Introsusception.

By John Coakley Lettsom, M. D. F. R. S. and A. S.

Read March 16, 1786.

A. B. a child four years old, was first indisposed about the middle of September, 1784. When I was consulted, which was on the 7th of October, the symptoms resembled those of a cholera morbus. At this period, however, the diarrhoea had ceased; but the patient continued frequently to vomit, especially after taking nourishment.

On the 20th a dysentery succeeded, with mucous and bloody stools, which ceased after a few days continuance, when she nearly recovered her former state of health, without ever reaching after her usual food. She was now removed into the country; and I did not hear of her again till December, when she was brought to town, on account of the return of the dysentery, which was, at this period, accompanied with a troublesome tenesmus, and a considerable degree of fever.

By anodyne medicines, and the use of opiate clysters, these complaints were occasionally moderated, and sometimes they totally ceased for a few days, but seldom longer, and the intervals of relief gradually shortened; the attacks became also more violent, commencing with violent rigors, and fever succeeding; the pulse grew weaker and weaker, and the patient became extremely extenuated in flesh; and towards the conclusion of this

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S f month,

month, after repeated vomitings of a dark-coloured fluid, like coffee grounds, it finished its painful existence.

Bleeding, before the debility was become alarming, afforded no material respite. Fomentations to the abdomen, and tepid bathing of the whole body, were equally ineffectual. Anodyne starch clysters afforded some truce, but it could not be durable; the nature of the mischief was too momentous to afford any hope of permanent relief, as the dissection after death will evince.

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bound down.

On a nicer inspection this arch was found to be a portion of the ileum, which passing within the band was inclosed in the sigmoid flexure of the colon.

All the parts between this portion of the small intestines and the sigmoid flexure, amongst which were the caput coli, cæcum with its appendix, and the whole of the great arch of the colon, could no where be seen. The want of these parts, the enlarged size of the sigmoid flexure, and the hard feel evidently shewing that it contained some substance, left no room to doubt, but that all the missing portion of the intestines

was contained within the sigmoid flexure. A finger introduced into the anus felt a round substance in the rectum, with an opening in the middle, not unlike the os tincæ. This substance did not adhere, the finger passing round it freely, between it and the internal coat of the rectum. The liver, the urinary bladder, and small intestines, were the remaining parts which first appeared when the parietes of the abdomen were turned back.

Upon looking for the omentum, a portion of it only was found attached to the stomach, the remaining part evidently passed within the band into the sigmoid flexure.

The stomach was tied much closer to the spine than natural, by the displacing of the omentum and great arch of the colon. The gall bladder was as large as that of an adult, and was full of thin bile, but without obstruction to its passage into the duodenum.

The general external appearance of all the intestines was natural, except slight inflammation in some places.

The cavity of the abdomen also contained more than half an ounce of thin pus; and on the right side were two ligamentous peritoneal substances, very much on the stretch; one formed by an extension of that part of the peritoneum called ligamentum * coli dextrum; the other at the place where the colon is connected to the peritoneum over the right kidney.

As the further investigation of this uncommon disease required particular attention, I cut out all the parts connected with it, bringing away the whole sigmoid flexure of the colon,

* I have observed, that in some children the caput coli is naturally connected much more loosely than in others. It is probable, that the present case was one of those.

with the rectum, anus, uterus, and bladder; also a part of the arch of the ileum with the omentum, and a portion of the stomach and duodenum.

The Drawing * (Tab. VII.) was taken by Mr. PONS, Surgeon, of the natural size, and the small intestines added from a sketch I made before the parts were removed from the body.

I then made a longitudinal incision through the coats of the sigmoid flexure of the colon, from the anus to the band at its upper part. Within the cavity, which was lined with mucus, appeared a large intestine, taking on the form of the sigmoid flexure, which on examination proved to be the great arch of the colon and the cæcum inverted; so that the villous coat was external, and in contact with the villous coat of the sigmoid flexure, through the whole extent of both; at the extremity of which inverted intestine were two apertures, viz. the large one felt by the finger *per anum*, and a smaller one.

It now plainly appeared, that the band was formed by the upper part of the sigmoid flexure being drawn tight by the inversion of the part of the colon immediately above it, the further progress of which was prevented by the peritoneal connections at that place not giving way; which caused it to appear as a band tying the intestine down.

This inclosed intestine was very much diseased, the upper part next the band being highly inflamed, and as it approached the caput coli in the rectum gradually terminated in mortification, so that for three inches from its extremity it was perfectly black.

No adhesion whatever appeared between the coats of these intestines, as this inverted colon might be lifted out of the sigmoid flexure to the band.

* Mr. BASIRE very accurately reduced the scale under my own inspection from which the engravings are taken.

Upon cutting through the coats of this inverted intestine it was observed, that they were very much thickened and diseased; the enlargement of the gut, which was fully equal to that of an adult, consisting chiefly in a thickening of its various muscular fibres *. The peritoneal coat, now become its internal surface, was every where highly inflamed, but not black as on the outside, the inflammation gradually increasing from the band to the extremity of the cæcum. Through the whole length of its cavity was included a portion of the ileum uninverted, with its connecting mesentery, which communicated with the larger aperture above described at the extremity of the cæcum, and with the arch of the ileum above the band. It was contracted in size, but was nearly free from thickening or inflammation; some adhesions only connected it with the coats of the colon; but the portion above the band was at least four times as large, thus resembling in magnitude as well as occupying the place of the great arch of the colon. Besides this intestine, this cavity contained a portion of the omentum continued from that above, passing within the band, and extending half-way to the rectum; an enlarged cluster of mesenteric glands, of the size of a pigeon's egg, which just emerged from under the band, and were connected with a portion of the mesentery above; and, at the lower part, the appendix vermiformis larger and longer than natural, but likewise uninverted, the mouth of the cavity of which formed the smaller opening in the cæcum before mentioned. It was at this point of the dissection that the same ingenious Surgeon drew the figure, tab. VIII.

* The increased action of these muscles, necessarily attendant on their inverted state, would increase the size of their muscular fibres, as happens in the bladder, when it acts frequently.

As long as the parts had been in this very uncommon situation, the fæces must have passed through the valve of the colon, directly into the very lowest part of the rectum, without ever coming in contact with any portion of the large intestines.

And in the last week of the child's life, when a prolapsus frequently happened, the fæces passed directly from the ileum into the night-stool.

The arch of the ileum, in default of that of the colon, formed the reservoir for the fæces; which, with the partial interruption to their passage by the stricture occasioned by the band, probably caused its enlargement. But the fæces contained in it were of a thinner consistence, and wanted the fætor usually met with in the colon.

EXPLANATION OF THE PLATES.

T A B. VII.

A general view of the intestines, in the situation in which they appeared on first opening the body,

aa. The enlarged ileum, putting on the appearance of the great arch of the colon.

b. The sudden enlargement of the ileum.

c. The ileum passing within the band into the colon.

d. Part of the omentum passing within the band.

e. The intestinal band, formed by the inversion of the great arch of the colon immediately above it ceasing at this place.

ff,

ff. The sigmoid flexure of the colon, containing the introsuscepted portion of the alimentary canal.

g. The rectum distended with the same.

h. The anus.

ii. Small intestines of the natural size and healthy appearance.

T A B. VIII.

The same view, with the sigmoid flexure laid open, and the edges turned back, to shew the contained parts; and likewise the introsuscepted colon laid open, to display the uninverted ileum and appendix vermiformis contained within it.

aaaa. Internal surface of the sigmoid flexure of the colon spread open.

bbbb. The external surface (by the inversion now become internal) of the great arch of the colon within the sigmoid flexure spread open.

cc. Part of the ileum uninverted.

d. Appendix cæci uninverted.

ee. A probe piercing the distended ileum, passed within the band, and brought out in another portion of the ileum, contained within the inverted colon below the band.

ff. A blow pipe passed through the valve of the colon, where it opened into the rectum, and brought out through the coats of the ileum above.

gg. A probe passed into the natural opening of the appendix cæci, and brought out above.

bb. The cæcum inverted.

surface of
the portion

XVI. *New Experiments on the Ocular Spectra of Light and Colours.* By Robert Waring Darwin, M. D.; communicated by Erasmus Darwin, M. D. F. R. S.

Read March 23, 1786.

WHEN any one has long and attentively looked at a bright object, as at the setting sun, on closing his eyes, or removing them, an image, which resembles in form the object he was attending to, continues some time to be visible: this appearance in the eye we shall call the ocular spectrum of that object.

These ocular spectra are of four kinds: 1st, Such as are owing to a less sensibility of a defined part of the retina; or *spectra from defect of sensibility*. 2d, Such as are owing to a greater sensibility of a defined part of the retina; or *spectra from excess of sensibility*. 3d, Such as resemble their object in its colour as well as form; which may be termed *direct ocular spectra*. 4th, Such as are of a colour contrary to that of their object; which may be termed *reverse ocular spectra*.

The laws of light have been most successfully explained by the great NEWTON, and the perception of visible objects has been ably investigated by the ingenious Dr. BERKELEY and M. MALEBRANCHE; but these minute phænomena of vision have yet been thought reducible to no theory, though many philosophers have employed a considerable degree of attention upon them: among these are Dr. JURIN, at the end of Dr. SMITH's Optics; M.

ÆPINUS, in the *Nov. Com. Petropol.* V. 10.; M. BEGUELIN, in the *Berlin Memoires*, V. II. 1771; M. D'ARCY, in the *Histoire de l'Acad. des Scienc.* 1765; M. DE LA HIRE; and, lastly, the celebrated M. DE BUFFON, in the *Memoires de l'Acad. des Scien.* who has termed them accidental colours, as if subjected to no established laws, *Ac. Par.* 1743. M. p. 215.

I must here apprise the reader, that it is very difficult for different people to give the same names to various shades of colours; whence, in the following pages, something must be allowed if, on repeating the experiments, the colours here mentioned should not accurately correspond with his own names of them.

I. ACTIVITY OF THE RETINA IN VISION.

From the subsequent experiments it appears, that the retina is in an active not in a passive state during the existence of these ocular spectra; and it is thence to be concluded, that all vision is owing to the activity of this organ.

1. Place a piece of red silk, about an inch in diameter, as in fig. 1. (Tab. IX.) on a sheet of white paper, in a strong light; look steadily upon it from about the distance of half a yard for a minute; then closing your eyelids cover them with your hands, and a green spectrum will be seen in your eyes, resembling in form the piece of red silk: after some time, this spectrum will disappear and shortly re-appear; and this alternately three or four times, if the experiment is well made, till at length it vanishes entirely.

2. Place on a sheet of white paper a circular piece of blue silk, about four inches in diameter, in the sunshine; cover the center of this with a circular piece of yellow silk, about three

three inches in diameter; and the center of the yellow silk with a circle of pink silk, about two inches in diameter; and the center of the pink silk with a circle of green silk, about one inch in diameter; and the center of this with a circle of indigo, about half an inch in diameter; make a small speck with ink in the very center of the whole, as in fig. 2.; look steadily for a minute on this central spot, and then closing your eyes, and applying your hand at about an inch distance before them, so as to prevent too much or too little light from passing through the eyelids, you will see the most beautiful circles of colours that imagination can conceive, which are most resembled by the colours occasioned by pouring a drop or two of oil on a still lake in a bright day; but these circular irises of colours are not only different from the colours of the silks above-mentioned, but are at the same time perpetually changing as long as they exist.

3. When any one in the dark presses either corner of his eye with his finger, and turns his eye away from his finger, he will see a circle of colours like those in a peacock's tail: and a sudden flash of light is excited in the eye by a stroke on it. (NEWTON's Opt. Qu. 16.)

4. When any one turns round rapidly on one foot, till he becomes dizzy and falls upon the ground, the spectra of the ambient objects continue to present themselves in rotation, or appear to librate, and he seems to behold them for some time still in motion.

From all these experiments it appears, that the spectra in the eye are not owing to the mechanical impulse of light impressed on the retina, nor to its chemical combination with that organ, nor to the absorption and emission of light, as is observed in many bodies: for in all these cases the spectra must

T t 2

either

either remain uniformly, or gradually diminish; and neither their alternate presence and evanescence as in the first experiment, nor the perpetual changes of their colours as in the second, nor the flash of light or colours in the pressed eye as in the third, nor the rotation or libration of the spectra as in the fourth, could exist.

It is not absurd to conceive, that the retina may be stimulated into motion, as well as the red and white muscles which form our limbs and vessels; since it consists of fibres, like those, intermixed with its medullary substance. To evince this structure, the retina of an ox's eye was suspended in a glass of warm water, and forcibly torn in a few places; the edges of these parts appeared jagged and hairy, and did not contract, and become smooth like simple mucus, when it is distended till it breaks; which shews that it consists of fibres; and this its fibrous construction became still more distinct to the sight, by adding some caustic alkali to the water, as the adhering mucus was first eroded, and the hair-like fibres remained floating in the vessel. Nor does the degree of transparency of the retina invalidate the evidence of its fibrous structure, since LEEUWENHOEK has shewn that the crystalline humour itself consists of fibres. (*Arcana Naturæ*, V. 1. p. 70.)

Hence it appears, that as the muscles have larger fibres intermixed with a smaller quantity of nervous medulla, the organ of vision has a greater quantity of nervous medulla intermixed with smaller fibres; and it is probable, that the locomotive muscles, as well as the vascular ones, of microscopic animals have much greater tenuity than these of the retina.

And besides the similar laws, which will be shewn in this Paper to govern alike the actions of the retina and of the muscles,

muscles, there are many other analogies which exist between them. They are both originally excited into action by irritations, both act nearly in the same quantity of time, are alike strengthened or fatigued by exertion, are alike painful if excited into action when they are in an inflamed state, are alike liable to paralysis, and to the torpor of old age.

II. OF SPECTRA FROM DEFECT OF SENSIBILITY.

The retina is not so easily excited into action by less irritation, after having been lately subjected to greater.

1. When any one passes from the bright daylight into a darkened room, the irises of his eyes expand themselves to their utmost extent in a few seconds of time; but it is very long before the optic nerve, after having been stimulated by the greater light of the day, becomes sensible of the less degree of it in the room; and, if the room is not too obscure, the irises will again contract themselves in some degree, as the sensibility of the retina returns.

2. Place about half an inch square of white paper on a black hat, and looking steadily on the center of it for a minute, remove your eyes to a sheet of white paper; and after a second or two a dark square will be seen on the white paper, which will continue some time. A similar dark square will be seen in the closed eye, if light be admitted through the eye-lids.

So after looking at any luminous object of a small size, as at the sun, for a short time, so as not much to fatigue the eyes, this part of the retina becomes less sensible to smaller quantities of light; hence, when the eyes are turned on other less luminous parts of the sky, a dark spot is seen resembling

the shape of the sun, or other luminous object which we last beheld. This is the source of one kind of the dark-coloured *muscæ volitantes*. If this dark spot lies above the center of the eye, we turn our eyes that way, expecting to bring it into the center of the eye, that we may view it more distinctly; and in this case the dark spectrum seems to move upwards. If the dark spectrum is found beneath the center of the eye, we pursue it from the same motive, and it seems to move downwards. This has given rise to various conjectures of something floating in the aqueous humours of the eyes; but whoever, in attending to these spots, keeps his eyes unmoved by looking steadily at the corner of a cloud, at the same time that he observes the dark spectra, will be thoroughly convinced, that they have no motion but what is given to them by the movement of our eyes in pursuit of them. Sometimes the form of the spectrum, when it has been received from a circular luminous body, will become oblong; and sometimes it will be divided into two circular spectra, which is owing to our changing the angle made by the two optic axes, according to the distance of the clouds or other bodies to which the spectrum is supposed to be contiguous. The apparent size of it will also be variable according to its supposed distance; but when such a spectrum is received with only one eye, the other being covered, its form and number are invariable.

As these spectra are more easily observable when our eyes are a little weakened by fatigue, it has frequently happened, that people of delicate constitutions have been much alarmed at them, fearing a beginning decay of their sight, and have thence fallen into the hands of ignorant oculists; but I believe they never are a prelude to any other disease of the eye, and that it is from habit alone, and our want of attention to them,

that

that we do not see them on all objects every hour of our lives. But as the nerves of very weak people lose their sensibility, in the same manner as their muscles lose their activity, by a small time of exertion, it frequently happens, that sick people in the extreme debility of fevers are perpetually employed in picking something from the bed-cloaths, occasioned by their mistaking the appearance of these *muscæ volitantes* in their eyes. BENVENUTO CELINI, an Italian artist, a man of strong abilities, relates, that having passed the whole night on a distant mountain with some companions and a conjurer, and performed many

chilliness on coming into an atmosphere of temperate warmth, after having been some time confined in a very warm room: and hence the stomach, and other organs of digestion, of those who have been habituated to the greater stimulus of spirituous liquor, are not excited into their due action by the less stimulus of common food alone; of which the immediate consequence is indigestion and hypochondriacism.

III. OF SPECTRA FROM EXCESS OF SENSIBILITY.

The retina is more easily excited into action by greater irritation after having been lately subjected to less.

1. If the eyes are closed, and covered perfectly with a hat, for a minute or two, in a bright day; on removing the hat a red or crimson light is seen through the eye-lids. In this experiment the retina, after being some time kept in the dark, becomes so sensible to a small quantity of light, as to perceive distinctly the greater quantity of red rays than of others which pass through the eye-lids. A similar coloured light is seen to pass through the edges of the fingers, when the open hand is opposed to the flame of a candle.

2. If you look for some minutes steadily on a window in the beginning of the evening twilight, or in a dark day, and then move your eyes a little, so that those parts of the retina, on which the dark frame-work of the window was delineated, may now fall on the glass part of it, many luminous lines, representing the frame-work, will appear to lie across the glass panes: for those parts of the retina, which were before least stimulated by the dark frame-work, are now more sensible to light

light than the other parts of the retina which were exposed to the more luminous parts of the window.

3. Make with ink on white paper a very black spot, about half an inch in diameter, with a tail about an inch in length, so as to represent a tadpole; look steadily for a minute on this spot, and, on moving the eye a little, the figure of the tadpole will be seen on the white part of the paper, which figure of the tadpole will appear whiter or more luminous than the other parts of the white paper; for the part of the retina on which the tadpole was delineated, is now more sensible to light than the other parts of it, which were exposed to the white paper. This experiment is mentioned by Dr. IRWIN, but is not by him ascribed to the true cause, namely, the greater sensibility of that part of the retina which has been exposed to the black spot, than of the other parts which had received the white field of paper, which is put beyond a doubt by the next experiment.

4. On closing the eyes after viewing the black spot on the white paper, as in the foregoing experiment, a red spot is seen of the form of the black spot: for that part of the retina, on which the black spot was delineated, being now more sensible to light than the other parts of it, which were exposed to the white paper, is capable of perceiving the red rays which penetrate the eyelids. If this experiment be made by the light of a tallow candle, the spot will be yellow instead of red; for tallow candles abound much with yellow light, which passes in greater quantity and force through the eyelids than blue light; hence the difficulty of distinguishing blue and green by this kind of candle light. The colour of the spectrum may possibly vary in the day light, according to the different colour of the meridian or the morning or evening light.

M. BEGUELIN, in the Berlin Memoires, V. II. 1771, observes, that, when he held a book so that the sun shone upon his half-closed eyelids, the black letters, which he had long inspected, became red, which must have been thus occasioned. Those parts of the retina which had received for some time the black letters, were so much more sensible than those parts which had been opposed to the white paper, that to the former the red light, which passed through the eyelids, was perceptible. There is a similar story told, I think, in M. DE VOLTAIRE's Historical Works, of a Duke of Tuscany, who was playing at dice with the general of a foreign army, and, believing he saw bloody spots upon the dice, portended dreadful events, and retired in confusion. The observer, after looking for a minute on the black spots of a die, and carelessly closing his eyes, on a bright day, would see the image of a die with red spots upon it, as above explained.

5. On emerging from a dark cavern, where we have long continued, the light of a bright day becomes intolerable to the eye for a considerable time, owing to the excess of sensibility existing in the eye, after having been long exposed to little or no stimulus. This occasions us immediately to contract the iris to its smallest aperture, which becomes again gradually dilated, as the retina becomes accustomed to the greater stimulus of the daylight.

The twinkling of a bright star, or of a distant candle in the night, is perhaps owing to the same cause. While we continue to look upon these luminous objects, their central parts gradually appear paler, owing to the decreasing sensibility of the part of the retina exposed to their light; whilst, at the same time, by the unsteadiness of the eye, the edges of them are perpetually falling on parts of the retina that were just before

before exposed to the darkness of the night, and therefore ten-fold more sensible to light than the part on which the star or candle had been for some time delineated. This pains the eye in a similar manner as when we come suddenly from a dark room into bright daylight, and gives the appearance of bright scintillations. Hence the stars twinkle most when the night is darkest, and do not twinkle through telescopes, as observed by MUSSCHENBROECK; and it will afterwards be seen why this twinkling is sometimes of different colours when the object is very bright, as Mr. MELVILL observed in looking at Sirius. For the opinions of others on this subject, see Dr. PRIESTLEY's valuable History of Light and Colours, p. 494.

Many facts observable in the animal system are similar to these; as the hot glow occasioned by the usual warmth of the air, or our cloaths, on coming out of a cold bath; the pain of the fingers on approaching the fire after having handled snow; and the inflamed heels from walking in snow. Hence those who have been exposed to much cold have died on being brought to a fire, or their limbs have become so much inflamed as to mortify. Hence much food or wine given suddenly to those who have almost perished by hunger has destroyed them; for all the organs of the famished body are now become so much more irritable to the stimulus of food and wine, which they have long been deprived of, that inflammation is excited, which terminates in gangrene or fever.

IV. OF DIRECT OCULAR SPECTRA.

A quantity of stimulus somewhat greater than natural excites the retina into spasmodic action, which ceases in a few seconds.

A certain duration and energy of the stimulus of light and colours excites the perfect action of the retina in vision; for very quick motions are imperceptible to us, as well as very slow ones, as the whirling of a top, or the shadow on a sundial. So perfect darkness does not affect the eye at all; and excess of light produces pain, not vision.

1. When a fire-coal is whirled round in the dark, a lucid circle remains a considerable time in the eye; and that with so much vivacity of light, that it is mistaken for a continuance of the irritation of the object. In the same manner, when a fiery meteor shoots across the night, it appears to leave a long lucid train behind it, part of which, and perhaps sometimes the whole, is owing to the continuance of the action of the retina after having been thus vividly excited. This is beautifully illustrated by the following experiment: fix a paper sail, three or four inches in diameter, and made like that of a smoke jack, in a tube of pasteboard; on looking through the tube at a distant prospect, some disjointed parts of it will be seen through the narrow intervals between the sails; but as the fly begins to revolve, these intervals appear larger; and when it revolves quicker, the whole prospect is seen quite as distinct as if nothing intervened, though less luminous.

2. Look through a dark tube, about half a yard long, at the area of a yellow circle of half an inch diameter, lying upon a blue area of double that diameter, for half a minute; and

and on closing your eyes the colours of the spectrum will appear similar to the two areas, as in fig. 3.; but if the eye is kept too long upon them, the colours of the spectrum will be the reverse of those upon the paper, that is, the internal circle will become blue, and the external area yellow; hence some attention is required in making this experiment.

3. Place the bright flame of a spermaceti candle before a black object in the night; look steadily at it for a short time, till it is observed to become somewhat paler; and on closing the eyes, and covering them carefully, but not so as to compress them, the image of the blazing candle will continue distinctly to be visible.

4. Look steadily, for a short time, at a window in a dark day, as in Exp. 2. S. III. and then closing your eyes, and covering them with your hands, an exact delineation of the window remains for some time visible in the eye. This experiment requires a little practice to make it succeed well; since, if the eyes are fatigued by looking too long on the window, or the day be too bright, the luminous parts of the window will appear dark in the spectrum, and the dark parts of the framework will appear luminous, as in Exp. 2. S. III. And it is even difficult for many, who first try this experiment, to perceive the spectrum at all; for any hurry of mind, or even too great attention to the spectrum itself, will disappoint them, till they have had a little experience in attending to such small sensations.

The spectra described in this section, termed direct ocular spectra, are produced without much fatigue of the eye; the irritation of the luminous object being soon withdrawn, or its quantity of light being not so great as to produce any degree of uneasiness in the organ of vision; which distinguishes them from

from the next class of ocular spectra, which are the consequence of fatigue. These direct spectra are best observed in such circumstances that no light, but what comes from the object, can fall upon the eye; as in looking through a tube, of half a yard long, and an inch wide, at a yellow paper on the side of a room, the direct spectrum was easily produced on closing the eye without taking it from the tube: but if the lateral light is admitted through the eye-lids, or by throwing the spectrum on white paper, it becomes a reverse spectrum, as will be explained below.

The other senses also retain for a time the impressions that have been made upon them, or the actions they have been excited into. So if a hard body is pressed upon the palm of the hand, as is practised in tricks of legerdemain, it is not easy to distinguish for a few seconds whether it remains or is removed; and tastes continue long to exist vividly in the mouth, as the smoke of tobacco, or the taste of gentian, after the sapid material is withdrawn.

v. *A quantity of stimulus somewhat greater than the last mentioned excites the retina into spasmodic action, which ceases and recurs alternately.*

1. On looking for a time on the setting sun, so as not greatly to fatigue the sight, a yellow spectrum is seen when the eyes are closed and covered, which continues for a time, and then disappears, and recurs repeatedly before it intirely vanishes. This yellow spectrum of the sun when the eye-lids are opened becomes blue; and if it is made to fall on the green grass, or on other coloured objects, it varies its own colour

colour by an intermixture of theirs, as will be explained in another place.

2. Place a lighted spermaceti candle in the night about one foot from your eye, and look steadily on the center of the flame, till your eye becomes much more fatigued than in S. IV. Exp. 3.; and on closing your eyes a reddish spectrum will be perceived, which will cease and return alternately.

The action of vomiting in like manner ceases, and is renewed by intervals, although the emetic drug is thrown up with the first effort: so after-pains continue some time after parturition; and the alternate pulsations of the heart of a viper are renewed for some time after it is cleared from its blood.

VI. OF REVERSE OCULAR SPECTRA.

The retina after having been excited into action by a stimulus somewhat greater than the last mentioned falls into opposite spasmodic action.

The actions of every part of animal bodies may be advantageously compared with each other. This strict analogy contributes much to the investigation of truth; while those looser analogies, which compare the phænomena of animal life with those of chemistry or mechanics, only serve to mislead our inquiries.

When any of our larger muscles have been in long or in violent action, and their antagonists have been at the same time extended, as soon as the action of the former ceases, the limb is stretched the contrary way for our ease, and a pandiculation or yawning takes place.

By the following observations it appears, that a similar circumstance obtains in the organ of vision; after it has been fatigued by one kind of action, it spontaneously falls into the opposite kind.

1. Place a piece of coloured silk, about an inch in diameter, on a sheet of white paper, about half a yard from your eyes; look steadily upon it for a minute; then remove your eyes upon another part of the white paper, and a spectrum will be seen of the form of the silk thus inspected, but of a colour opposite to it. A spectrum nearly similar will appear if the eyes are closed, and the eyelids shaded by approaching the hand near them, so as to permit some but to prevent too much light falling on them.

Red silk produced a green spectrum.

Green produced a red one.

Orange produced blue.

Blue produced orange.

Yellow produced violet.

Violet produced yellow.

That in these experiments the colours of the spectra are the reverse of the colours which occasioned them, may be seen by examining the third figure in Sir ISAAC NEWTON's Optics, L. II. p. 1. where those thin laminæ of air, which reflected yellow, transmitted violet; those which reflected red, transmitted a blue-green; and so of the rest, agreeing with the experiments above related.

2. These reverse spectra are similar to a colour, formed by a combination of all the primary colours except that with which the eye has been fatigued in making the experiment: thus the reverse spectrum of red must be such a green as would be produced by a combination of all the other prismatic colours.

To

To evince this fact the following satisfactory experiment was made. The prismatic colours were laid on a circular pasteboard wheel, about four inches in diameter, in the proportions described in Dr. PRIESTLEY's History of Light and Colours, pl. 12. fig. 83. except that the red compartment was intirely left out, and the others proportionably extended so as to complete the circle. Then, as the orange is a mixture of red and yellow, and as the violet is a mixture of red and indigo, it became necessary to put yellow on the wheel instead of orange, and indigo instead of violet, that the experiment might more exactly quadrate with the theory it was designed to establish or confute; because in gaining a green spectrum from a red object, the eye is supposed to have become insensible to red light. This wheel, by means of an axis, was made to whirl like a top; and on its being put in motion, a green colour was produced, corresponding with great exactness to the reverse spectrum of red.

3. In contemplating any one of these reverse spectra in the closed and covered eye, it disappears and re-appears several times successively, till at length it intirely vanishes, like the direct spectra in sect. v.; but with this additional circumstance, that when the spectrum becomes faint or evanescent, it is instantly revived by removing the hand from before the eyelids, so as to admit more light: because then not oaly the fatigued part of the retina is inclined spontaneously to fall into motions of a contrary direction, but being still sensible to all other rays of light, except that with which it was lately fatigued, is by these rays at the same time stimulated into those motions which form the reverse spectrum.

From these experiments there is reason to conclude, that the fatigued part of the retina throws itself into a contrary mode

of action, like oscitation or pandiculation, as soon as the stimulus which has fatigued it is withdrawn; and that it still remains sensible, that is, liable to be excited into action by any other colours at the same time, except the colour with which it has been fatigued.

VII. *The retina after having been excited into action by a stimulus somewhat greater than the last mentioned falls into various successive spasmodic actions.*

1. On looking at the meridian sun as long as the eyes can well bear its brightness, the disc first becomes pale, with a luminous crescent, which seems to librate from one edge of it to the other, owing to the unsteadiness of the eye; then the whole phasis of the sun becomes blue, surrounded with a white halo; and on closing the eyes, and covering them with the hands, a yellow spectrum is seen, which in a little time changes into a blue one.

M. DE LA HIRE observed, after looking at the bright sun, that the impression in his eye first assumed a yellow appearance, and then green, and then blue; and wishes to ascribe these appearances to some affection of the nerves. (PORTERFIELD on the Eye, Vol. I. p. 343.)

2. After looking steadily on about an inch square of pink silk, placed on white paper, in a bright sunshine, at the distance of a foot from my eyes, and closing and covering my eyelids, the spectrum of the silk was at first a dark green, and the spectrum of the white paper became of a pink. The spectra then both disappeared; and then the internal spectrum was blue; and then, after a second disappearance, became yellow, and

and lastly pink, whilst the spectrum of the field varied into red and green.

These successions of different coloured spectra were not exactly the same in the different experiments, though observed, as near as could be, with the same quantity of light, and other similar circumstances; owing, I suppose, to trying too many experiments at a time; so that the eye was not quite free from the spectra of the colours which were previously attended to.

The alternate exertions of the retina in the preceding section resembled the oscitation or pandiculation of the muscles, as they were performed in directions contrary to each other, and were the consequence of fatigue rather than of pain. And in this they differ from the successive dissimilar exertions of the retina, mentioned in this section, which resemble in miniature the more violent agitations of the limbs in convulsive diseases, as epilepsy, chorea S. Viti, and opisthotonos; all which diseases are perhaps, at first, the consequence of pain, and have their periods afterwards established by habit.

VIII. *The retina, after having been excited into action by a stimulus, somewhat greater than the last mentioned, falls into a fixed spasmodic action, which continues for some days.*

1. After having looked long at the meridian sun, in making some of the preceding experiments, till the discs faded into a pale blue, I frequently observed a bright blue spectrum of the sun on other objects all the next and the succeeding day, which constantly occurred when I attended to it, and frequently when I did not previously attend to it. When I closed and covered

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my eyes, this appeared of a dull yellow; and at other times mixed with the colours of other objects on which it was thrown. It may be imagined, that this part of the retina was become insensible to white light, and thence a bluish spectrum became visible on all luminous objects; but as a yellowish spectrum was also seen in the closed and covered eye, there can remain no doubt of this being the spectrum of the sun. A similar appearance was observed by M. *ÆPINUS*, which he acknowledges he could give no account of. (Nov. Com. Petrop. V. 10, p. 2. and 6.)

The locked jaw, and some cataleptic spasms, are resembled by this phenomenon; and from hence we may learn the danger to the eye by inspecting very luminous objects too long a time.

IX. A quantity of stimulus greater than the preceding induces a temporary paralysis of the organ of vision.

1. Place a circular piece of bright red silk, about half an inch in diameter, on the middle of a sheet of white paper; lay them on the floor in a bright sunshine, and fixing your eyes steadily on the center of the red circle, for three or four minutes, at the distance of four or six feet from the object, the red silk will gradually become paler, and finally cease to appear red at all.

2. Similar to these are many other animal facts; as purges, opiates, and even poisons, and contagious matter, cease to stimulate our system, after we have been habituated to their use. So some people sleep undisturbed by a clock, or even by a forge hammer in their neighbourhood: and not only continued irritations, but violent exertions of any kind, are succeeded by

by temporary paralysis. The arm drops down after violent action, and continues for a time useless; and it is probable, that those who have perished suddenly in swimming, or in skating on the ice, have owed their deaths to the paralysis, or extreme fatigue, which succeeds every violent and continued exertion.

X. MISCELLANEOUS REMARKS.

There were some circumstances occurred in making these experiments, which were liable to alter the results of them, and which I shall here mention for the assistance of others, who may wish to repeat them.

1. *Of direct and inverse spectra existing at the same time; of reciprocal direct spectra; of a combination of direct and inverse spectra; of a spectral halo; rules to pre-determine the colours of spectra.*

a. When an area, about six inches square, of bright pink Indian paper, had been viewed on an area, about a foot square, of white writing paper, the internal spectrum in the closed eye was green, being the reverse spectrum of the pink paper; and the external spectrum was pink, being the direct spectrum of the pink paper. The same circumstance happened when the internal area was white, and external one pink; that is, the internal spectrum was pink, and the external one green. All the same appearances occurred when the pink paper was laid on a black hat.

b. When six inches square of deep violet polished paper was viewed on a foot square of white writing paper, the internal spectrum

spectrum was yellow, being the reverse spectrum of the violet paper, and the external one was violet, being the direct spectrum of the violet paper.

c. When six inches square of pink paper was viewed on a foot square of blue paper, the internal spectrum was blue, and the external spectrum was pink; that is, the internal one was the direct spectrum of the external object, and the external one was the direct spectrum of the internal object, instead of their being each the reverse spectrum of the objects they belonged to.

d. When six inches square of blue paper were viewed on a foot square of yellow paper, the interior spectrum became a brilliant yellow, and the exterior one a brilliant blue. The vivacity of the spectra was owing to their being excited both by the stimulus of the interior and exterior objects; so that the interior yellow spectrum was both the reverse spectrum of the blue paper, and the direct one of the yellow paper; and the exterior blue spectrum was both the reverse spectrum of the yellow paper, and the direct one of the blue paper.

e. When the internal area was only a square half-inch of red paper, laid on a square foot of dark violet paper, the internal spectrum was green, with a reddish-blue halo. When the red internal paper was two inches square, the internal spectrum was a deeper green, and the external one redder. When the internal paper was six inches square, the spectrum of it became blue, and the spectrum of the external paper was red.

f. When a square half-inch of blue paper was laid on a six-inch square of yellow paper, the spectrum of the central paper in the closed eye was yellow, incircled with a blue halo. On looking long on the meridian sun, the disc fades into a pale blue surrounded with a whitish halo.

These circumstances, though they very much perplexed the experiments till they were investigated, admit of a satisfactory explanation; for while the rays from the bright internal object in exp. *a.* fall with their full force on the center of the retina, and, by fatiguing that part of it, induce the reverse spectrum, many scattered rays, from the same internal pink paper, fall on the more external parts of the retina, but not in such quantity as to occasion much fatigue, and hence induce the direct spectrum of the pink colour in those parts of the eye. The same reverse and direct spectra occur from the violet paper in exp. *b.*: and in exp. *c.* the scattered rays from the central pink paper produce a direct spectrum of this colour on the external parts of the eye, while the scattered rays from the external blue paper produce a direct spectrum of that colour on the central part of the eye, instead of these parts of the retina falling reciprocally into their reverse spectra. In exp. *d.* the colours being the reverse of each other, the scattered rays from the exterior object falling on the central parts of the eye, and there exciting their direct spectrum, at the same time that the retina was excited into a reverse spectrum by the central object, and this direct and reverse spectrum being of similar colour, the superior brilliancy of this spectrum was produced. In exp. *e.* the effect of various quantities of stimulus on the retina, from the different respective sizes of the internal and external areas, induced a spectrum of the internal area in the center of the eye, combined of the reverse spectrum of that internal area and the direct one of the external area, in various shades of colour, from a pale green to a deep blue, with similar changes in the spectrum of the external area. For the same reasons, when an internal bright object was small, as in exp. *f.*, instead of the whole of the spectrum of the external object, being,

being reverse to the colour of the internal object, only a kind of halo, or radiation of colour, similar to that of the internal object, was spread a little way on the external spectrum. For this internal blue area being so small, the scattered rays from it extended but a little way on the image of the external area of yellow paper, and could therefore produce only a blue halo round the yellow spectrum in the center.

If any one should suspect that the scattered rays from the exterior coloured object do not intermix with the rays from the interior coloured object, and thus affect the central part of the eye, let him look through an opaque tube, about two feet in length, and an inch in diameter, at a coloured wall of a room with one eye, and with the other eye naked; and he will find, that by shutting out the lateral light, the area of the wall seen through a tube appears as if illuminated by the sunshine, compared with the other parts of it; from whence arises the advantage of looking through a dark tube at distant paintings.

Hence we may safely deduce the following rules to determine before-hand the colours of all spectra. 1. *The direct spectrum* without any lateral light is an evanescent representation of its object in the unfatigued eye. 2. With some lateral light it becomes of a colour combined of the direct spectrum of the central object, and of the circumjacent objects, in proportion to their respective quantity and brilliancy. 3. *The reverse spectrum* without lateral light is a representation in the fatigued eye of the form of its objects, with such a colour as would be produced by all the primary colours, except that of the object. 4. With lateral light the colour is compounded of the reverse spectrum of the central object, and the direct spectrum of the circumjacent objects, in proportion to their respective quantity and brilliancy.

II. Variation and vivacity of the spectra occasioned by extraneous light.

The reverse spectrum, as has been before explained, is similar to a colour, formed by a combination of all the primary colours, except that with which the eye has been fatigued in making the experiment: so the reverse spectrum of red is such a green as would be produced by a combination of all the other prismatic colours. Now it must be observed, that this reverse spectrum of red is therefore the direct spectrum of a combination of all the other prismatic colours, except the red; whence, on removing the eye from a piece of red silk to a sheet of white paper, the green spectrum, which is perceived, may either be called the reverse spectrum of the red silk, or the direct spectrum of all the rays from the white paper, except the red; for in truth it is both. Hence we see the reason why it is not easy to gain a direct spectrum of any coloured object in the day-time, where there is much lateral light, except of very bright objects, as of the setting sun, or by looking through an opaque tube; because the lateral external light falling also on the central part of the retina, contributes to induce the reverse spectrum, which is at the same time the direct spectrum of that lateral light, deducting only the colour of the central object which we have been viewing. And for the same reason, it is difficult to gain the reverse spectrum, where there is no lateral light to contribute to its formation. Thus, in looking through an opaque tube on a yellow wall, and closing my eye, without admitting any lateral light, the spectra were all at first yellow; but at length changed into blue. And on looking in the same manner on red paper, I did at length get a

Vol. LXXVI. Y y green.

green spectrum; but they were all at first red ones: and the same after looking at a candle in the night.

The reverse spectrum was formed with greater facility when the eye was thrown from the object on a sheet of white paper, or when light was admitted through the closed eyelids; because not only the fatigued part of the retina was inclined spontaneously to fall into motions of a contrary direction; but being still sensible to all other rays of light except that with which it was lately fatigued, was by these rays stimulated at the same time into those motions which form the reverse spectrum. Hence, when the reverse spectrum of any colour became faint, it was wonderfully revived by admitting more light through the eyelids, by removing the hand from before them: and hence, on covering the closed eyelids, the spectrum would often cease for a time, till the retina became sensible to the stimulus of the smaller quantity of light, and then it recurred. Nor was the spectrum only changed in vivacity, or in degree, by this admission of light through the eyelids; but it frequently happened, after having viewed bright objects, that the spectrum in the closed and covered eye was changed into a third spectrum, when light was admitted through the eyelids: which third spectrum was composed of such colours as could pass through the eyelids, except those of the object. Thus, when an area of half an inch diameter of pink paper was viewed on a sheet of white paper in the sunshine, the spectrum with closed and covered eyes was green; but on removing the hands from before the closed eyelids, the spectrum became yellow, and returned instantly again to green, as often as the hands were applied to cover the eyelids, or removed from them: for the retina being now insensible to red light, the yellow rays passing through the eyelids in greater quantity than the other colours, induced a yellow spectrum; whereas if
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the spectrum was thrown on white paper, with the eyes open, it became only a lighter green.

Though a certain quantity of light facilitates the formation of the reverse spectrum, a greater quantity prevents its formation, as the more powerful stimulus excites even the fatigued parts of the eye into action; otherwise we should see the spectrum of the last viewed object as often as we turn our eyes. Hence the reverse spectra are best seen by gradually approaching the hand near the closed eyelids to a certain distance only, which must be varied with the brightness of the day, or the energy of the spectrum. Add to this, that all dark spectra, as black, blue, or green, if light be admitted through the eyelids, after they have been some time covered, give reddish spectra, for the reasons given in sect. III. exp. 1.

From these circumstances of the extraneous light coinciding with the spontaneous efforts of the fatigued retina to produce a reverse spectrum, as was observed before, it is not easy to gain a direct spectrum, except of objects brighter than the ambient light; such as a candle in the night, the setting sun, or viewing a bright object through an opaque tube; and then the reverse spectrum is instantaneously produced by the admission of some external light; and is as instantly converted again to the direct spectrum by the exclusion of it. Thus, on looking at the setting sun, on closing the eyes, and covering them, a yellow spectrum is seen, which is the direct spectrum of the setting sun; but on opening the eyes on the sky, the yellow spectrum is immediately changed into a blue one, which is the reverse spectrum of the yellow sun, or the direct spectrum of the blue sky, or a combination of both. And this is again transformed into a yellow one on closing the eyes, and so reciprocally, as quick as the motions of the opening and closing eyelids. Hence, when Mr. MELVILL observed

the scintillations of the star Sirius to be sometimes coloured, these were probably the direct spectrum of the blue sky on the parts of the retina fatigued by the white light of the star. (*Essays Physical and Literary*, p. 81. V. 2.)

When a direct spectrum is thrown on colours darker than itself, it mixes with them; as the yellow spectrum of the setting sun, thrown on the green grass, becomes a greener yellow. But when a direct spectrum is thrown on colours brighter than itself, it becomes instantly changed into the reverse spectrum, which mixes with those brighter colours. So the yellow spectrum of the setting sun thrown on the luminous sky becomes blue, and changes with the colour or brightness of the clouds on which it appears. But the reverse spectrum mixes with every kind of colour on which it is thrown, whether brighter than itself or not: thus the reverse spectrum, obtained by viewing a piece of yellow silk, when thrown on white paper was a lucid blue green; when thrown on black Turkey leather becomes a deep violet. And the spectrum of blue silk, thrown on white paper, was a light yellow; on black silk was an obscure orange; and the blue spectrum, obtained from orange-coloured silk, thrown on yellow, became a green.

In these cases the retina is thrown into activity or sensation by the stimulus of external colours, at the same time that it continues the activity or sensation which forms the spectra; in the same manner as the prismatic colours, painted on a whirling top, are seen to mix together. When these colours of external objects are brighter than the direct spectrum which is thrown upon them, they change it into the reverse spectrum, like the admission of external light on a direct spectrum, as explained above. When they are darker than the direct spectrum,

trum, they mix with it, their weaker stimulus being insufficient to induce the reverse spectrum.

III. *Variation of spectra in respect to number and figure and remission.*

When we look long and attentively at any object, the eye cannot always be kept intirely motionless; hence, on inspecting a circular area of red silk placed on white paper, a lucid crescent or edge is seen to librate on one side or other of the red circle: for the exterior parts of the retina sometimes falling on the edge of the central silk, and sometimes on the white paper, are less fatigued with red light than the central part of the retina, which is constantly exposed to it; and therefore, when they fall on the edge of the red silk, they perceive it more vividly. Afterwards, when the eye becomes fatigued, a green spectrum in the form of a crescent is seen to librate on one side or other of the central circle, as by the unsteadiness of the eye a part of the fatigued retina falls on the white paper; and as by the increasing fatigue of the eye the central part of the silk appears paler, the edge on which the unfatigued part of the retina occasionally falls will appear of a deeper red than the original silk, because it is compared with the pale internal part of it. M. DE BUFFON in making this experiment observed, that the red edge of the silk was not only deeper coloured than the original silk; but, on his retreating a little from it, it became oblong, and at length divided into two, which must have been owing to a change of the angle of the two optic axes with the new distance he observed it at. Thus, if a pen is held up before a distant candle, when we look intently at the pen two candles are seen behind it; when we look intently at

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the candle two pens are seen. If the sight be unsteady at the time of beholding the sun, even though one eye only be used, many images of the sun will appear, or luminous lines, when the eye is closed. And as some parts of these will be more vivid than others, and some parts of them will be produced nearer the center of the eye than others, these will disappear sooner than the others; and hence the number and shape of these spectra of the sun will continually vary, as long as they exist. The cause of some being more vivid than others, is the unsteadiness of the eye of the beholder, so that some parts of the retina have been longer exposed to the sunbeams. That some parts of a complicated spectrum fade and return before other parts of it, the following experiment evinces. Draw three concentric circles; the external one an inch and a half in diameter, the middle one an inch, and the internal one half an inch; colour the external and internal areas blue, and the remaining one yellow, as in fig. 4.; after having looked about a minute on the center of these circles, in a bright light, the spectrum of the external area appears first in the closed eye, then the middle area, and lastly the central one; and then the central one disappears, and the others in inverted order. If concentric circles of more colours are added, it produces the beautiful ever changing spectrum in sect. I. exp. 2.

From hence it would seem, that the center of the eye produces quicker remissions of spectra, owing perhaps to its greater sensibility; that is, to its more energetic exertions. These remissions of spectra bear some analogy to the tremors of the hands, and palpitations of the heart, of weak people: and perhaps a criterion of the strength of any muscle or nerve may be taken from the time it can be continued in exertion.

IV. *Variation of spectra in respect to brilliancy; the visibility of the circulation of the blood in the eye..*

1. The meridian or evening light makes a difference in the colours of some spectra; for as the sun descends, the red rays, which are less refrangible by the convex atmosphere, abound in great quantity. Whence the spectrum of the light parts of a window at this time, or early in the morning, is red; and becomes blue either a little later or earlier; and white in the meridian day; and is also variable from the colour of the clouds or sky which are opposed to the window.

2. All these experiments are liable to be confounded, if they are made too soon after each other, as the remaining spectrum will mix with the new ones. This is a very troublesome circumstance to painters, who are obliged to look long upon the same colour; and in particular to those whose eyes, from natural debility, cannot long continue the same kind of exertion. For the same reason, in making these experiments, the result becomes much varied if the eyes, after viewing any object, are removed on other objects for but an instant of time, before we close them to view the spectrum; for the light from the object, of which we had only a transient view, in the very time of closing our eyes acts as a stimulus on the fatigued retina; and for a time prevents the desired spectrum from appearing, or mixes its own spectrum with it. Whence, after the eyelids are closed, either a dark field, or some unexpected colours, are beheld for a few seconds, before the desired spectrum becomes distinctly visible.

3. The length of time taken up in viewing an object, of which we are to observe the spectrum, makes a great difference
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in the appearance of the spectrum, not only in its vivacity, but in its colour; as the direct spectrum of the central object, or of the circumjacent ones, and also the reverse spectra of both, with their various combinations, as well as the time of their duration in the eye, and of their remissions or alternations, depend upon the degree of fatigue the retina is subjected to. The Chevalier D'ARCY constructed a machine by which a coal of fire was whirled round in the dark, and found, that when a luminous body made a revolution in eight thirds of time, it presented to the eye a complete circle of fire; from whence he concludes, that the impression continues on the organ about the seventh part of a second. (*Mém. de l'Acad. des Sc.* 1765.) This, however, is only to be considered as the shortest time of the duration of these direct spectra; since in the fatigued eye both the direct and reverse spectra, with their intermissions, appear to take up many seconds of time, and seem very variable in proportion to the circumstances of fatigue or energy.

4. It sometimes happens, if the eyeballs have been rubbed hard with the fingers, that lucid sparks are seen in quick motion amidst the spectrum we are attending to. This is similar to the flashes of fire from a stroke on the eye in fighting, and is resembled by the warmth and glow which appear upon the skin after friction, and is probably owing to an acceleration of the arterial blood into the vessels emptied by the previous pressure. By being accustomed to observe such small sensations in the eye, it is easy to see the circulation of the blood in this organ. I have attended to this frequently, when I have observed my eyes more than commonly sensible to other spectra. The circulation may be seen either in both eyes at a time, or only in one of them; for as a certain quantity of light is necessary to

to produce this curious phænomenon, if one hand be brought nearer the closed eyelids than the other, the circulation in that eye will for a time disappear. For the easier viewing the circulation, it is sometimes necessary to rub the eyes with a certain degree of force after they are closed, and to hold the breath rather longer than is agreeable, which, by accumulating more blood in the eye, facilitates the experiment; but in general it may be seen distinctly after having examined other spectra with your back to the light, till the eyes become weary; then having covered your closed eyelids for half a minute, till the spectrum is faded away which you were examining, turn your face to the light, and removing your hands from the eyelids, by and by again shade them a little, and the circulation becomes curiously distinct. The streams of blood are however generally seen to unite, which shews it to be the venous circulation, owing, I suppose, to the greater opacity of the colour of the blood in these vessels; for this venous circulation is also much more easily seen by the microscope in the tail of a tadpole.

v. Variation of spectra in respect to distinctness and size; with a new way of magnifying objects.

1. It was before observed, that when the two colours viewed together were opposite to each other, as yellow and blue, red and green, &c. according to the table of reflections and transmissions of light in Sir ISAAC NEWTON's Optics, B. II. fig. 3. the spectra of those colours were of all others the most brilliant, and best defined; because they were combined of the reverse spectrum of one colour, and of the direct spectrum of the other. Hence, in books printed with small types, or in

the minute graduation of thermometers, or of clock-faces, which are to be seen at a distance, if the letters or figures are coloured with orange, and the ground with indigo; or the letters with red, and the ground with green; or any other lucid colour is used for the letters, the spectrum of which is similar to the colour of the ground; such letters will be seen much more distinctly, and with less confusion, than in black or white: for as the spectrum of the letter is the same colour with the ground on which they are seen, the unsteadiness of the eye in long attending to them will not produce coloured lines by the edges of the letters, which is the principal cause of their confusion. The beauty of colours lying in vicinity to each other, whose spectra are thus reciprocally similar to each colour, is owing to this greater ease that the eye experiences in beholding them distinctly; and it is probable, in the organ of hearing a similar circumstance may constitute the pleasure of melody. Sir ISAAC NEWTON observes, that gold and indigo were agreeable when viewed together; and thinks there may be some analogy between the sensations of light and sound. (Optics, Qu. 14.)

In viewing the spectra of bright objects, as of an area of red silk of half an inch diameter on white paper, it is easy to magnify it to tenfold its size: for if, when the spectrum is formed, you still keep your eye fixed on the silk area, and remove it a few inches further from you, a green circle is seen round the red silk: for the angle now subtended by the silk is less than it was when the spectrum was formed, but that of the spectrum continues the same, and our imagination places them at the same distance. Thus when you view a spectrum on a sheet of white paper, if you approach the paper to the eye, you may diminish it to a point; and if the paper is made to recede from

the eye, the spectrum will appear magnified in proportion to the distance.

I was surprised, and agreeably amused, with the following experiment. I covered a paper about four inches square with yellow, and with a pen filled with a blue colour wrote upon the middle of it the word BANKS in capitals, as in fig. 5. and, sitting with my back to the sun, fixed my eyes for a minute exactly on the center of the letter N in the middle of the word; after closing my eyes, and shading them somewhat with my hand, the word was distinctly seen in the spectrum in yellow letters on a blue field; and then, on opening my eyes on a yellowish wall at twenty feet distance, the magnified name of BANKS appeared written on the wall in golden characters.

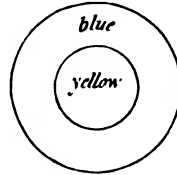
CONCLUSION.

It was observed by the learned M. SAUVAGES (Nofol. method. Cl. VIII. Ord. 1.) that the pulsations of the optic artery might be perceived by looking attentively on a white wall well illuminated. A kind of net-work, darker than the other parts of the wall, appears and vanishes alternately with every pulsation. This change of the colour of the wall he well ascribes to the compression of the retina by the diastole of the artery. The various colours produced in the eye by the pressure of the finger, or by a stroke on it, as mentioned by Sir ISAAC NEWTON, seem likewise to originate from the unequal pressure on various parts of the retina. Now as Sir ISAAC NEWTON has shewn, that all the different colours are reflected or transmitted by the laminæ of soap bubbles, or of air, according to their different thickness or thinness, is it not probable, that the

effect of the activity of the retina may be to alter its thickness or thinness, so as better to adapt it to reflect or transmit the colours which stimulate it into action? May not muscular fibres exist in the retina for this purpose, which may be less minute than the locomotive muscles of microscopic animals? May not these muscular actions of the retina constitute the sensation of lights and colours; and the voluntary repetitions of them, when the object is withdrawn, constitute our memory of them? And lastly, may not the laws of the sensations of light, here investigated, be applicable to all our other senses, and much contribute to elucidate many phænomena of animal bodies both in their healthy and diseased state; and thus render this investigation well worthy the attention of the physician, the metaphysician, and the natural philosopher?

Derby, November 1, 1785.

Fig. 3.



yellow

BANKS.

The letters blue.

XVII. *Observations on some Causes of the Excess of the Mortality of Males above that of Females.* By Joseph Clarke, M. D. Physician to the Lying-in Hospital at Dublin. Communicated by the Rev. Richard Price, D. D. F. R. S. in a Letter to Charles Blagden, M. D. Sec. R. S.

Read March 30, 1786.

S I R,

Newington-Green, February 6, 1786.

I RECEIVED some time ago the inclosed letters and registry from Dr. CLARKE, Physician to the Lying-in Hospital at Dublin. They contain some accounts that seem to me not improper to be communicated to the Royal Society.

The observations which have been made on the laws that govern human mortality prove, that the mortality of males exceeds that of females in almost all the stages of life, and particularly in the earliest stages; and that this excess prevails most in great towns, and all the less natural situations of human life. The facts in these papers throw some light on this subject. Male *fætus's* requiring more nutrition than female *fætus's*, because larger, and being also for this reason more liable to injury in delivery, are brought into the world less perfect: and this happening more or less in proportion to the vigour and just formation of the mother, it must happen most in those situations where the greatest tenderness of frame and deviations from nature take place. The truth, in short,

short, seems to be, that any debility in *either* parent must affect most the production of that sex which requires the largest and strongest *flamina*; and that such debilities prevailing most in great towns and polished Societies, the excess of the mortality of males must also be greatest in such situations. And this I reckon the principal reason of a circumstance in human mortality which, before I received these communications from Dr. CLARKE, I did not so well understand.

With much respect I am, &c.

RICH. PRICE.

Dr. CLARKE's first Letter to the Rev. Dr. PRICE.

S I R,

Dublin.

IN your very useful Treatise on Life Annuities, &c. you remark *, that “ it has been observed, that the Author of “ nature has provided, that more *males* should be born than “ females, on account of the particular waste of males, occasioned by wars and other causes. That perhaps it might “ have been observed, with more reason, that this provision “ had in view that particular weakness or delicacy in the constitution of males which makes them more subject to mortality; and which, consequently, renders it necessary that “ more of them should be produced, in order to preserve

* Vol. I. p. 373.

“ in

"in the world a due proportion between the sexes." And further, you elsewhere remark *, that "the *facts* recited at the end of your fourth Essay *prove*, that there is a difference between the mortality of males and females; but that you must however observe, that it may be *doubted*, whether this difference, so unfavourable to males, be *natural*; and that there are facts which prove that you have reason for such a doubt." After stating a number of very satisfactory facts of this kind you remark, that "the inference from them is very obvious; that they seem to shew sufficiently, that human life in males is more brittle than in females, only in consequence of adventitious causes, or of some particular debility which takes place in polished and luxurious societies, and especially in great towns."

What those adventitious causes are, or how this particular debility is produced and operates, are questions which appear to me highly interesting and curious. I have therefore been at considerable pains to examine and arrange a very accurate and extensive registry in such a manner as I hope will throw some light on these questions. As it is to the accuracy of modern registers that we are originally indebted for our knowledge of the facts in question, I apprehend, it is from the same source only that we shall be enabled satisfactorily to explain them.

Of the registry inclosed, I beg leave to observe to you, Sir, that it has been kept from its commencement by a man of uncommon accuracy (one of the under-clerks of our House of Commons); and that as the poor women and their children are obliged to pass through his office, before leaving the Hospital, his situation is such that there is no likelihood of his being deceived. It exhibits to our view the occurrences of 28 years

* Vol. II. p. 247.

in above 20,000 instances: a number which I am inclined to think can hardly appear insufficient for establishing some general inferences and conclusions on a tolerably sure foundation. Although my reasoning on these matters should not appear very conclusive, or my calculations perfectly accurate, yet I flatter myself, that the facts will neither be unacceptable nor useless to you.

I believe it may be safely asserted, that anatomy has not hitherto detected any internal difference between the animal œconomy of the male and female, which can be supposed to account for their difference of mortality, more especially in early Infancy; and this (it deserves to be particularly remarked) is the period during which the chances are much the greatest against male life. It is a matter of common observation that *males, cæteris paribus*, grow to a greater size than females, both *in utero* and every subsequent period of their growth. Consequently, they must meet with more difficulty, and endure more hardship and fatigue, in the hour of birth. Accordingly, practitioners in midwifery, taught by experience, know, that when any considerable difficulty occurs in the birth of a child (for example, in all the different kinds of preternatural labours) they stand a much better chance of saving the life of a female than of a male. It is on this principle we can explain what our registry concurs with others in proving, *viz.* that near one-half more males than females are still-born. Naturalists are agreed, that the head of the human foetus is larger in proportion to its body than that of any other animal; and I believe it is certain, that no animal whatever brings forth its young with so much difficulty, pain, and danger, as a woman. Now as we know that the head contains one of the most important organs of the body to life, it is highly reasonable to suppose, that any additional

additional injury which it sustains in delivery may produce very material effects on the whole system. These effects though often may not be always immediate. They may operate in weakening the male constitution so as to render it more apt to be affected by any exciting cause of disease soon after birth, and less able to struggle against it. It may be asked, how this will apply to the difference of mortality in great towns and country situations? The answer evidently is, that in great towns rickets, scrophula, and other diseases affecting the bones, and producing consequent mal-conformation of the female sex, are more frequent than in healthy country situations.

There is another circumstance, Sir, which may have some influence in producing that particular *debility* which you mention. It is this: as the stamina of the male are naturally constituted to grow to a greater size, a greater supply of nourishment *in utero* will be necessary to his growth than to that of a female. Defects in this particular, proceeding from delicacy of constitution or diseases of the mother, must of course be more injurious to the male sex. And although the male children may be so lucky as to escape abortion and the perils of delivery, it is probable, that they will be more apt to languish under disease, or die at some future period, from the application of noxious causes to an originally half-starved frame. To a person little accustomed to consider physiological subjects, this reasoning may appear somewhat obscure. It may, perhaps, be somewhat illustrated by considering that nourishment of the *fœtus* *after* birth which nature has provided for. Suppose every mother in a great city obliged to suckle and nurse her own child, *without* the assistance of spoon-meat; and every mother in the adjacent country to do the same. Of the former there would not perhaps be one *good* nurse in *five*; and of the

latter, perhaps, *not one* bad in *ten*. The difference of mortality that would ensue both to mothers and children thus situated, and the greater sufferings of the male than female sex, may be easily conceived, but not easily calculated. We see that, when a woman conceives twins, and has two foetuses *in utero* to nourish instead of one, it becomes peculiarly fatal both to her and her offspring. The chances are above four to one greater against her than against a woman bringing forth one child, and about two to one against her issue*.

Give me leave, Sir, to call your attention a little further to the facts relating to twins. They are singular and curious, at the same time that they serve to confirm some of the preceding reasoning. Near *one-half* more twins die, and near *one-third* more are still-born, than of single children. And why?—It is not because they meet with greater difficulties in the birth. On the contrary, it is a known fact, that, being much less than other children, women bring them forth with more ease. Does it not then proceed from a scanty nutrition, by which they are oftener blighted *in utero* than single children; and, when born alive, have less strength to support life through the first stages of its existence.

It is farther worthy of observation, that though *double* the numbers of twins die and are still-born, compared to single children, yet the proportion of male twins lost to females is *less*. Only one-fifth more of the male sex die than of the female, and only one-third more is still-born. Whereas of single children, whose proportional mortality is one-half less, *one-fourth* more of the male sex die, and near double the number is still-born. To what then are we to attribute this lessened mortality in favour of male twins? Probably to their brain and

* Compare the 7th and 14th, 6th and 13th inferences in the annexed extracts.

nervous

nervous system suffering less during delivery, on account of their heads being much smaller than those of single children. Were I disposed to be prolix, I could offer many more plausible arguments on this subject; but to you, Sir, I am sure they would be unnecessary. There is only one circumstance remaining, relative to the proportion of the sexes, which I cannot pass over in silence. We see evident wisdom in the creation of a greater number of males than females; but why the proportion they bear to each other differs in different countries and situations, and why there should be a seventeenth more males born of single children than twins, are questions which I leave to be decided by those philosophers who understand the theory of generation better than I do. Be this as it may, I am convinced that the majority in favour of the male sex is sooner destroyed than the generality of writers seem to be aware of. Did the limits of this letter permit, I think, I could prove from Dr. SHORT's own data*, that the majority of males is destroyed long before the common marriageable period; but I shall content myself with an observation or two on the registry before us. If one-half of the whole born in this hospital die before three years, which is the established computation for great cities; and if, on the loss of somewhat more than a *third* of this half, a majority of 1177 be reduced to 483 by a loss of 694, as appears from the registry, it is pretty evident, that by the death of the two remaining thirds, a majority will be left in favour of the female sex. It is obvious, that the statement with regard to twins corroborates this supposition; for of them, instead of a fifth, there is near one *half* dead and still-born, the consequence of which is; that we send out a majority of females. It may be objected, that their males do

* New Observations, p. 72. et seq.

not bear so great a proportion to the females; and that, therefore, it is not to be expected they should keep up their majority so long. But there is only a seventeenth fewer males produced; whereas it has been already shewn, that there is a much greater proportion between the deaths of single and twin males against the former and in favour of the latter.

Such are the outlines, Sir, of my sentiments on this subject. I have assumed the liberty of addressing them to you without ceremony, as a well-wisher to every member of the republic of letters. I shall be happy, should your sentiments happen to coincide with mine, or if I can be of any farther service in promoting your very laudable inquiries.

I am, Sir, with great respect, &c.

JOSEPH CLARKE

Lying-in Hospital,
June 9, 1785.

Dr. CLARKE's second Letter to the Rev. Dr. PRICE

S I R,

Dublin, Oct. 22, 1785.

ENCOURAGED by your approbation of my former letter, I will take the liberty of stating to you a few more facts and observations, which I hope you will judge an Appendix to it of some importance.

With the view of ascertaining how far some of the foregoing conjectures are well founded, and of determining with greater

greater precision the more obvious differences between the male and female sex in infancy, I began in the month of July last by weighing forty children, twenty of each sex, and by taking the dimensions of their heads. In the months of August and September I repeated the same experiment twice, taking such children as appeared to have arrived at the full period of gestation promiscuously as they happened to be born.

I weighed them all a few hours after birth, before they had taken food, and before purgative medicines had time to operate. For this purpose, I made use of a small spring or pocket steelyard, which weighs anything (not heavier than a few pounds) appended to it with sufficient accuracy. To this was attached a flannel bag, into which the children were put, at first, naked; but this I soon found very troublesome. The nurses often wanted time sufficient to assist me, and timid mothers were afraid of their infants catching cold; I was therefore obliged to weigh them with their cloaths on, and to subtract a certain quantity from the gross weight of each child, according as it was full, middling, or light clothed. Whatever inaccuracy this may have introduced, as to the real weight of the children, it can but little influence their comparative weights, or the differences between the two sexes, which it was my object to ascertain.

For measuring their heads, I made use of a piece of painted or varnished linen tape, divided into inches, halves, and quarters. The varnish has the good effect of preventing the length of such a measure being readily affected by variations in the humidity of the atmosphere, &c.; and it has little or no elasticity. In this part of the experiment then I can pretend to considerable accuracy. I took first the greatest circumference of the head from the most prominent part of the occiput around over the frontal sinuses; and, secondly, the transverse dimension.

dimension from the superior and anterior part of one ear, across the fontanelle, to a similar part of the opposite ear. These dimensions appeared to me the most likely to afford data for determining the respective sizes of the brain in the different sexes. The result was as follows :

Twenty males.			Twenty females.		
Weight. lbs. &c.	Circumference of heads. Inches.	Dimensions from ear to ear. Inches.	Weight. lbs. &c.	Circumf. of heads. Inches.	Dimen. from ear to ear. Inches.
Experiment 1.					
149½	282	152	137½	272	143
Experiment 2.					
144½	277	146½	135	272	147
Experiment 3.					
148	280	147½	132	273	143½
Totals.					
442	839	445½	404½	817	433½
Average weight, &c.					
7 lbs. 5 oz. 7 dr.	14	7½	6 lbs. 11 oz. 6 dr.	13½	7½

Having found the relative proportions between the sexes to turn out thrice with so much uniformity, and observing them to correspond pretty nearly with some experiments, made for very different purposes by the late Professor ROEDERER, of Gottingen, I did not think it necessary to prosecute the subject farther.

Upon the whole, it may be observed, that the difference of weight between the male and female at birth may be rated at about nine ounces, or nearly a twelfth part of the original weight. In the circumference of their heads there is a difference of near half an inch, or about a 28th or 30th part; and the same proportion of a 28th is pretty nearly preserved in the transverse dimension. It is evident, as the bony passage through

through which infants pass is of a certain determined capacity, that, were their heads equally incompressible with those of adults, the difference of half an inch in their size would often prove fatal to them. By the compressibility of their heads, however, in *well formed* women, this difficulty is by time surmounted. The effects which such a compression on the *brain* may produce, have not hitherto been well attended to.

In reckoning children, weighing from $5\frac{1}{2}$ to $6\frac{1}{2}$, 6 pounds weight, and from $6\frac{1}{2}$ to $7\frac{1}{2}$, 7, and so forth, in order to avoid fractions, I find the numbers of males and females, arranged according to their weight, to stand as follow.

Males.								Females.							
lbs.	4	5	6	7	8	9	10	lbs.	4	5	6	7	8	9	10
Nº	0	3	6	32	16	2	1	Nº	2	9	14	25	8	2	0

Hence it appears, that the majority of males runs thus: seven, eight, six, five; whilst that of the females is seven, six, five, eight. Hence also appears the merciful dispensations of Providence towards the female sex; for when deviations from the medium standard occur, it is remarkable, that they are much more frequently below than above this standard. In 120 instances there are only five children exceeding eight pounds and a half in weight. The same may be observed with regard to the size of their heads. Only six measured above $14\frac{1}{2}$ inches in circumference, and these all of the male sex; five measured $14\frac{1}{2}$, and one 15. In transverse dimensions only four exceeded $7\frac{1}{2}$, the largest of which was $8\frac{1}{2}$; whereas deviations under the standard in these particulars were very numerous, never however under 12 around and $6\frac{1}{2}$ across.

In

In the year 1753, Dr. ROEDERER published a Paper, *De pondere et longitudine Infantum recens natorum*, in the Commentaries of the Royal Society of Gottingen, of which the celebrated HALLER was the principal institutor, and long the president. In this Paper he proves, in the clearest manner, by incontestible experiments, the absurdity of the ideas of obstetric writers with regard to the progress of the ovum during gestation, and the weight of the foetus after birth. He shews, although they state the weight of the foetus, come to the full time, to be from 12 to 14 or 16 pounds, that it is more generally 6 or 7, and very rarely exceeds eight. This deserves particular notice for two reasons; first, because it serves to shew how little dependence is to be placed on the assertions of authors who copy each other servilely, without having recourse to experiment even in the most obvious cases; and, secondly, because this paper has been overlooked by some of the most celebrated writers and teachers of midwifery now living. What idea are we to form of the accuracy of one of our latest systematic writers, who (telling us that he has been a practitioner of midwifery, in a capital city, for twenty years, and a teacher for more than twelve) states, in one page of his work, that the weight of a foetus at eight months is about seven pounds; and on the opposite page, that at full time it weighs from twelve to fourteen pounds*?

Of 27 children, carried to the full period of gestation, weighed and measured in length by ROEDERER, without any attention to the difference of sex, I find, that 18 were of the male and 9 of the female sex; and that the average weight of

* See a Treatise of Midwifery (p. 88. and 89.) divested of technical terms and *abstruse theories*, by A. HAMILTON, M. D. 8^o edit. London, 1781.

the former was about 6 lbs. 9 oz., that of the latter about 6 lbs. 2 oz. 2 dr. Whether he and I used the same weights, I cannot exactly say. He observes, that he used the civil pound of Gottingen, which I can easily perceive consisted of 16 ounces, as mine did; but whether a German ounce be the same with ours, I have not *data* to determine. The average length of the males measured by him is about $20\frac{1}{4}$ inches, and of the females about $19\frac{1}{4}$. He weighed also the placenta of 21 lying-in women, 16 of whom had borne male children, and five female. The average weight of the former was 1 lb. $2\frac{1}{4}$ oz.; that of the latter 1 lb. 2 oz. Hence it appears, that in other circumstances, besides those I have taken notice of, the male and female sex differ. So far I thought it necessary to take extracts from Dr. ROEDERER's paper, as his observations and mine throw light on each other, and add confirmation to both.

The limits of this letter will not permit me, Sir, to trespass much farther on your patience. There is one circumstance or two so intimately connected with my former letter, that I cannot pass them over in silence. Having found that males suffer more in the birth than females, I was desirous of knowing whether the chance of the mother's recovery was thereby in any degree affected; and to determine this I was once more at the pains of turning over our registry with care. I found, that of 214 women, dead of single children, 50 were delivered of still-born males, and 15 of still-born females; 76 of living males, and 73 of living females. Of the 15 dead of twins, 6 had twins one of each sex; 6 others had twins both of the male sex; and three had twins both of the female sex. All of which twins (two or three excepted), it is very remarkable, survived the death of their mothers. It would appear then, that the life of the mother is principally endan-

gered in those cases where the bulk of the male's head precludes the possibility of his being brought into the world alive, either by the efforts of nature or art. The conception of twins we have observed to be more fatal to the mother than that of single children. The average weight of 12 twins, which have occurred to me of late, I find to be 11 lbs. a pair. The largest pair weighed 13 lbs. and the least 8½. From some rude attempts made to ascertain the weight of the contents of the gravid uterus in cases of twin and single children, I am inclined to think, that they are to each other as about 15 to 10, or perhaps 14½ to 9½.

Believe me, Sir, with great respect, &c.

J. CLARKE.

An Abstract of the Registry kept at the Lying-in Hospital, in Dublin, from the 8th of December, 1784. By B. H. Register.

From 8th to 31st of December, 1757	Number of Patients admitted	Went out not delivered	Delivered in the Hospital.	Boys born	Girls born.	Total number of children.
1758	55	1	55	36	25	55
1759	455	7	454	255	207	462
1760	413	15	406	228	192	420
1761	571	16	556	300	260	560
1762	537	17	521	283	249	532
1763	550	31	533	279	266	545
1764	519	22	488	274	224	498
1764	610	22	588	287	308	595

OF 20117 children born, at the end of a fortnight, there

Balance 495 in favour of the male sex.

B b b 3

women having twins, as *one* to about *sixty*.
women dying in child-bed, as *one* to about *eighty-seven*;

n the year 1757 to 1784.

parous, Twins, Triplets, &c.
Children.

Sex.		Dead.		Still-born.	
M.	F.	M.	F.	M.	F.
342	320	116	91	29	20
320		91		20	
<hr/>		<hr/>		<hr/>	
662		207		49	
		49			
		<hr/>			

Total 256 dead and still-born.

twins to females born	.	.	17 to 16
dying under 16 days	.	.	1 to 3½
still-born	.	.	1 to 13½
twins dying to females	.	.	5 to 4
— still-born to ditto	.	.	3 to 2
born and dead of each sex to the whole			1 to 2½
in dying	.	.	1 to 22

Totals of twins, &c. dead and still-born.

	Males.	Females.
	116	91
	29	20
	<hr/>	<hr/>
	145	111
Born	342	320
Dead and still-born	145	111
	<hr/>	<hr/>
Sent out living	197	209
		197

XVIII. *Some Particulars of the present State of Mount Vesuvius; with the Account of a Journey into the Province of Abruzzo, and a Voyage to the Island of Ponza. In a Letter from Sir William Hamilton, K. B. F. R. S. and A. S. to Sir Joseph Banks, Bart. P. R. S.*

Read May 4, 1786.

S I R,

Naples, January 24, 1786.

THE eruption of Mount Vesuvius, which began in the month of November 1784, nearly at the moment of my return from England to this Capital, and which continued in some degree till about the 20th of last month, has afforded much amusement to travellers unacquainted with this wonderful operation of nature, but no new circumstance that could justify my troubling you with a letter on the subject. The lava either overflowed the rim of the crater, or issued from small fissures on its borders, on that side which faces the mountain of Somma, and ran more or less in one, and at times in three or four channels, regularly formed, down the flanks of the conical part of the volcano; sometimes descending and spreading itself in the valley between the two mountains; and once, when the eruption was in its greatest force, in the month of November last, the lava descended still lower, and did some damage to the vineyards, and cultivated parts at the foot of Vesuvius, towards the village of St. Sebastiano; but geperally

B b b 3

the

the lava, not being abundant, stopped and cooled before it was able to reach the valley. By the accumulation of these lava's on the flanks of Vesuvius, its form has been greatly altered; and by the frequent explosion of scorix and ashes, a considerable mountain has been formed within the crater, which now rising much above its rim has likewise given that part of the mountain a new appearance. Just before I left Naples, in May 1783, I was at the top of Vesuvius. The crater was certainly then more than 250 feet deep, and was impracticable, its sides being nearly perpendicular. This eruption, however, has been as satisfactory as could be desired by the inhabitants of this city, a prodigious quantity of lava having been disgorged; which matter, confined within the bowels of the earth, would probably have occasioned tremors; and even slight ones might prove fatal to Naples, whose houses are, in general, very high, ill built, and a great number in almost every street already supported by props, having either suffered by former earthquakes, or from the loose volcanic soil's having been washed from under their foundations by the torrents of rain water from the high grounds which surround Naples, and on which a great part of the town itself is built.

From the time of the last formidable eruption of Mount Vesuvius, in August 1779 (described in one of my former communications to the Royal Society) to this day, I have, with the assistance of the Father Antonio Piaggi *, kept an exact diary of the operations of Vesuvius, with drawings, shewing, by the quantity of smoke, the degrees of fermenta-

* This Padre Antonio Piaggi is the ingenious Monk who invented the method of unfolding and recovering the burnt ancient manuscripts of Herculaneum, and who resides constantly at Refina, at the foot, and in full view, of Mount Vesuvius.

tion of the volcano; also the course of the lava's during this last eruption, and the changes that have been made in the form of the mountain itself by the lava's and scorizæ that have been ejected. This journal is becoming very curious and interesting; it is remarkably so with respect to the pointing out a variety of singular effects that different currents of air have upon the smoke that issues from the crater of Vesuvius, elevated (as you know, Sir) more than 3600 feet above the level of the sea; but, except the smoke increasing considerably and constantly when the sea is agitated, and the wind blows from that quarter, the operations of Vesuvius appear to be very capricious and uncertain. One day there will be the appearance of a violent fermentation, and the next all is calmed again: but whenever the smoke has been attended with considerable ejections of scorizæ and cinders, I have constantly observed, that the lava has soon after made its appearance, either by boiling over the crater, or forcing its passage through crevices in the conical part of the volcano. As long as I remain in this country, and have the necessary assistance of the above-mentioned ingenious Monk (who is as excellent a draughtsman as he is an accurate and diligent observer) the Vesuvian diary shall be continued; and I hope one day to have the honour of presenting these curious manuscripts (which begin now to be voluminous) to the Royal Society, if it should think them worthy of a place in the Library of the Society.

Having never had an opportunity of examining the islands of Ponza, Palmarole, Zannone, and other small islands, or rather rocks, situated between the island of Ventotiene and Monte Circello, near Terracina, on the Continent; and thinking that by a tour of these islands I should be enabled to render my former observations more complete, and to communicate
to

to you, Sir, some account of the only volcanic parts of this neighbourhood hitherto undescribed, I determined to take advantage of the absence of their Sicilian Majesties (who were then making the tour of Italy) and visit these islands. But before I put this plan in execution, I made a long excursion in the province of Abruzzo, as far as the Lake of Celano, anciently called Fucinus, and where the famous Emissary of the Emperor CLAUDIUS (a most stupendous work * for draining that lake) remains nearly entire, though filled up with rubbish and earth in many parts, and of course useless. The water of this lake, which is more than 30 miles in circumference, increases daily, and is destroying the rich and cultivated plains on its borders. It is surrounded by very high mountains, many of them covered with snow, and at the foot of them are many villages, and rich and well cultivated farms. Upon the whole it is the most beautiful lake I ever saw, and would be complete, if the neighbouring mountains were better wooded. This lake furnishes abundance of fish, but not of the best quality: a few large trout, but mostly tench, barbel, and dace. In the shallow water on the borders of the lake, I saw thousands of water snakes, pursuing and preying upon a little fish like our thornbacks, but much better armed, though their defensive weapons seemed to avail them but little against such ravenous foes.

I went with torches into the emissary of CLAUDIUS as far as I could. It is a covered under-ground canal, three miles long, and great part of it cut through a hard rock; the other parts supported by masonry, with wells sunk to give air and light.

* A description of this emissary of CLAUDIUS, with plans (though not very exact) has been published by FABRETTI, in the same book in which he has given an account of TRAJAN's column.

According

According to SÆTONIUS, CLAUDIUS employed thirty thousand men eleven years on this great work, intended to convey the superfluous water of the lake into the bed of the river Liris, now called Garigliano; and I make no doubt, but that if it was cleared and repaired, it would again answer that purpose.

In its present state it is a most magnificent monument of antiquity.

The whole country from Arpino, the native place of MARIUS*, by Isola, Sora, Civitella, and Capistrello, to the lake of Celano, is, in my opinion, infinitely more beautiful and picturesque than any spot I have yet seen on the Alps, in Savoy, Switzerland, or the Tyrol. The road is not passable for carriages, and indeed is scarcely so, even in summer, for horses or mules, and is often infested with banditti; a party of which, consisting of twenty-two, had quartered themselves in a village which I passed through, and left it but a week before my arrival. There are many wolves and some bears in the adjacent mountains, which also commit their depredations in the winter. The tyger-cat, *gatto pardo*, or lynx, is sometimes found in the woods of this part of Abruzzo.

The road follows the windings of the Garigliano, which is here a beautiful clear trout stream, with a great variety of cascades and water-falls, particularly a double one at Isola, near which place CICERO had a villa, and there are still some remains of it, though converted to a chapel. The valley is extensive, and rich with fruit trees, corn, vines, and olives. Large tracts of land are here and there covered with woods of

* MARIUS had a large villa, about twelve miles distant from Arpino. I went to visit the spot, on which now stands the only convent of the austere order of La Trappe in Italy. It is in the Pope's state, and has been evidently built of the ruins of MARIUS's house, and its present name is *Casa Mari*.

oak and chestnut, all timber trees of the largest size. The mountains nearest the valley rise gently, and are adorned with either modern castles, towns, and villages, or the ruins of ancient ones. The next range of mountains, rising behind these, are covered with pines, larches, and such trees and shrubs as usually abound in a like situation: and above them a third range of mountains and rocks, being the most elevated part of the Apennine, rise much higher, and, being covered with eternal snow, make a beautiful contrast with the rich valley above-mentioned; and the snow is at so great a distance, as not to give that uncomfortable chill to the air, which I have always found in the narrow vallies of the Alps and the Tyrol. Excuse me, Sir, if from the impression which this enchanting and little frequented country has left on my mind, I have been led to depart from the subject of this letter, to which I will return directly.

On the 15th of August last I went in a felucca to the island of Ischia. I have nothing to add to my former observations on this island, already communicated to the Royal Society; except that about sixty yards from the shore, at a place called St. Angelo, situated between the towns of Ischia and Furia, a column of boiling water bubbles upon the surface of the sea with great force, and communicates its heat to the water of the sea near it; but as the wind was very high, and the surf considerable, I was not able then to examine this curious spot as I could have wished, but will return there on purpose some other time. The inhabitants of the neighbourhood told me, that it always boiled up in the same manner, winter and summer; and that it was of great use to them in bending their planks for ship-building; and that the fishermen also frequently made use of this natural cauldron to boil their fish. Though I
have

have passed at different times many weeks in the island of Ischia, I never before heard of this phænomenon; but in my description of this island mention is made of several spots where, near the shore, I had found, when bathing in the sea, the sand under my feet so hot as to oblige me to retire hastily. This boiling spring reminds me of one near Viterbo in the Roman State, which I have seen, and is called the Bulicame. It is a circular pool of about sixty feet in diameter, and exceedingly deep, the water of which is constantly boiling. It is situated in a plain surrounded by volcanic mountains. A stony concretion floats on the surface of the pool, which being carried off by the superfluous water is deposited, and is constantly forming a labes or tuffa, of which all the soil around the pool is composed. You have seen, Sir, the like operation in greater perfection in Iceland, at the famous boiling spring of Geyser. I am convinced, that many of the finer sort and most compact tuffa's we meet with, in countries formed by volcanoes, have been produced in the same manner.

The 18th of August I arrived at the island of Ventotiene, about twenty-five miles from Ischia. It is greatly improved since my former visit, seven or eight years ago, when his Sicilian Majesty first planted a little colony there. It then produced neither corn nor wine; now it furnishes annually at least seventy butts of wine and two thousand *tomoli* of corn. The soil is remarkably fertile, from whence it probably took its ancient Greek name of Pandataria. This island contains at present more than three hundred inhabitants. The island of Ventotiene, and the smaller one called St. Stefano, within a mile of it, having been described in my *Campi Phlegræi*, as being both entirely composed of volcanic matter, I need not trouble you further on their subject; I will only mention a

curious circumstance in the natural history of birds, of which I was informed by an officer of the garrison of Ventotiene, who is a great sportsman, and shoots often in the island of St. Stefano, inhabited only by hawks, and a large kind of sea-gulls; but is occasionally visited, as a resting place, by divers sorts of birds of passage. In the month of May great flights of quails arrive there from Africa, spent with fatigue; and many of them fall an easy prey for the hawks and sea-gulls; but, as their arrival depends upon one prevailing wind, there is often an interval of many days between one flight and another. My informer assured me, that the hawks constantly, during the flights, make a provision of each day's prey, laying them up in separate heaps of six or seven near their haunts, always feeding first upon those of the oldest date. The sea-gulls have not the same foresight, but greedily fall upon their unhappy victims in their languid state before they reach the shore, and, having beat them down into the sea, swallow numbers of them whole. Extraordinary as this may appear, yet as facts related by persons of credibility in any branch of natural history are always pleasing, I thought you would excuse this digression. Give me leave likewise to add, for the information of the curious in antiquities, that, during my stay in the island of Ventotiene, I got out of the ruins of an elegant ancient bath (supposed to have been built for the use of JULIA, daughter of AUGUSTUS, whilst she was in exile here) a fragment of a tile, on which are stamped the following characters in basso relievo,

HACINI
IVLIAI
AVGVS. F

which, according to the interpretation of a celebrated antiquary at Naples, mean *Opus HACINI ad commodum Balnei JULIÆ AVGVSTÆ factum*. I was informed, that several entire tiles,

tiles, with a like inscription, had been dug up on the same spot, and had been made use of in building the church and barracks newly erected in this island. Another fragment of a tile was likewise found here, and given to me, with the following inscription :

SAB. A PI.

which the same antiquary explains, SABINAE AVGVSTAE, Pia Imperatrici *dicatum Balneum* ; but, I believe, there is no mention in ancient authors of SABINA having been at Pandataria : of JULIA's banishment to this island there can be no doubt.

Between Ventotiene and the island of Ponza, and from the latter at the distance of about twelve miles, a group of rocks rise several feet above the surface of the sea. They are called the *Botte*, and are composed of a compact lava ; probably they are the small remains of another volcanic island, the softer parts of which may have been carried off and levelled by the action of the sea, which is open and violent here.

The 20th of August I arrived at the island of Ponza, about thirty miles from Ventotiene, and the next day I went round it in my boat. It is near five miles long : its greatest breadth not more than half a mile, and in some parts not more than five hundred feet. It is surrounded by innumerable detached rocks, some of them very high, and most of which are of lava ; in many are regularly formed basalttes, but none in large columns. In some parts the basalttes have a reddish tint of iron ochre, are very small, and irregularly laid one over another. Some masses of them are in a perpendicular, others in an horizontal, and others again in an inclined position : and the rocks themselves, in which these masses are found, are lava of the same nature as the basalttes. At first sight these rocks have very much the appearance of the ruins of ancient Roman

C c c 2

brick

brick or rather tile buildings, as may be seen in the drawing (see Tab. XI. fig. 1.) taken on the spot. One rock, as appears in the drawing (see Tab. XII. fig. 4.) is composed of large spherical basaltic; and in many parts of the island I found the lava had inclined to take the like spherical form, though on a much smaller scale, some of the first mentioned round basaltic being near two feet in diameter. All these rocks have certainly been detached by the action of the sea from the island, which is entirely composed of volcanic matter, lava's, and tuffa's, of various qualities and tints, green, yellow, black, and white. Some of the tuffa's, as well as the lava's, are of a texture more compact than others; and in some parts of the island great tracts seem to have undergone the same operation as is mentioned in one of my former communications to be in full force at a spot called the Pisciarelli, on the outside of the Solfaterra, near Puzzole, and where a hot sulphureous vitriolic acid vapour converts all which it penetrates, whether lava's, tuffa's, volcanic ashes, or pumice stones, into a pure clay, mostly white, or with a light tint of red, blue, green, or yellow. The appearance of a tract of volcanic country, which has undergone this operation, is well expressed in the view of the inside of the harbour of Ponza (Tab. XI. fig. 2.). But I was so struck with the beautiful and uncommon appearance of one of these high volcanic grounds converted to a pure light-coloured clay (Tab. XII. fig. 1.) in contrast with a neighbouring dark basaltic rock, that I caused the drawing, which accompanies this letter (see Tab. XII.) to be made on the spot. You, Sir, who have seen such a variety of countries, will still think this view singular and beautiful. I can assure you, it is very exact, except the rock of round basaltic (fig. 4.) which, in nature, is at a distance from this spot, and only placed here
to

to illustrate what I have written on its subject. In one part of the island there is a sort of tuffa, remarkably good for the purpose of building. It is as hard as our Bath stone, and nearly of the same colour, without any mixture of fragments of lava or pumice stone, which usually abound in the tuffa's in the neighbourhood of Naples, Baia, and Puzzole.

The drawing (see Tab. XI.), which is a view of the harbour of Ponza, will give you a very good idea of the appearance of the isolated rocks of lava and basalt which have been separated, by the force of the sea, from the softer parts of the island, and of which there are an infinite number, as you will see in the exact geometrical plan of the island of Ponza (Tab. X.), which likewise accompanies this letter.

When I was last in England, I inquired of many of the manufacturers of glass, whether it had ever happened, that the glass cooling in their furnaces had taken any distinct forms like prisms or crystallizations; but I got no satisfactory answer until I applied to the ingenious Mr. PARKER, of Fleet-street, who not only informed me, that, some years ago, a quantity of his flint glass had been rendered unserviceable by taking such a form in cooling; but also gave me several curious specimens of the glass itself: some of them are in laminæ, which may be easily separated; and others resemble basaltic columns in miniature, having regular faces. I was much pleased with this discovery, proving to me, beyond a doubt, the volcanic origin of most basaltes. Many of the rocks of lava of the island of Ponza are, with respect to their configurations, strikingly like the specimens of Mr. PARKER's above-mentioned glass, none being very regularly formed basaltes, but all having a tendency towards it. Mr. PARKER could not account for the accident that occasioned his glass to take the basaltic forms;

forms; but I have remarked, both in Sicily and at Naples, that such lava's as have run into the sea, are either formed into regular basaltcs, or have a great tendency towards such a form. The lava's of Mount Etna, which ran into the sea near Iacci, as appears in my account of them in the *Campi Philegræi*, are perfect basaltcs; and a lava that ran into the sea from Mount Vesuvius, near Torre del Greco, in 1631, has an evident tendency to the basaltic forms. On Mount Vesuvius I never found any thing like columns of basaltcs, except the above-mentioned at Torre del Greco, and some fragments of very complete ones, which I picked up near the crater, after the eruption of 1779, and which had been thrown out of the mouth of the volcano.

The island of Palmarole, which is about four miles from Ponza, is not much more than a mile in circumference, is composed of the same volcanic matter, and probably was once a part of Ponza; and indeed it appears as if the island of Zannone, which lies at about the same distance from the island of Ponza, was once likewise a part of the same island of Ponza; for many rocks of lava rise above water in a line between the two last mentioned islands, and the water is much shallower there than in the other parts of the gulph of Terracina.

The island of Zannone is larger and much higher than Palmarole, and the half of the island nearest the Continent is composed of a lime-stone, exactly similar to that of the Apennines, on the Continent near it; the other half is composed of lava's and tuffa's, resembling in every respect the soil of the other islands just described. Neither Palmarole nor Zannone are inhabited; but the latter furnishes brushwood in abundance for the use of the inhabitants of Ponza, whose number, including the garrison, amounts to near seventeen hundred.

The

The uninhabited island of St. Stefano furnishes fuel in the like manner for the inhabitants of Ventotiene.

It is probable, that all these islands and rocks may in time be levelled by the action of the sea. Ponza, in its present state, is the mere skeleton of a volcanic island, as little more than its harder vitrified parts remain, and they seem to be slowly and gradually mouldering away. Other new volcanic islands may likewise be produced in these parts.

The gulphs of Gaeta and Terracina may, in the course of time, become another Campo Felice *; for, as has been mentioned in one of my former communications on this subject, the rich and fertile plain so called, which extends from the bay of Naples to the Apennines, behind Caserta and Capua, has evidently been intirely formed by a succession of such volcanic eruptions. Vesuvius, the Solfaterra, and the high volcanic ground, on which great part of this city is built, were once probably islands; and we may conceive, the islands of Procita, Ischia, Ventotiene, Palmarole, Ponza, and Zannone, to be the outline of a new portion of land, intended by nature to be added to the neighbouring Continent; and the Lipari islands (all of which are volcanic) may be looked upon in the same light with respect to a future intended addition of territory to the island of Sicily. If you cast your eye, Sir, on the map at the head of my description of the Campi Phlegræi, you will better understand my meaning.

* The governor of the castle of Ponza, who has resided there fifty-three years, told me, that the island was still subject to earthquakes; that there had been one violent shock there about four years ago; but that the most violent one he ever felt there was on the very day and hour of the great earthquake which destroyed Lisbon; that two houses out of three, which were then on the island, were thrown down. This seems to prove, that the volcanic matter, which gave birth to these islands, is not exhausted.

The

The more opportunities I have of examining this volcanic country, the more I am convinced of the truth of what I have already ventured to advance, which is, that volcanoes should be considered in a creative rather than a destructive light. Many new discoveries have been made of late years, particularly, as you well know, Sir, in the South-Seas, of islands which owe their birth to volcanic explosions; and some, indeed, where the volcanic fire still operates. I am led to believe, that upon further examination, most of the elevated islands at a considerable distance from Continents would be found to have a volcanic origin; as the low and flat islands appear in general to have been formed of the spoils of sea productions, such as corals, madrepores, &c. But I will stop here, and not deviate from the plan which I have hitherto strictly followed, of reporting faithfully to my learned Brethren of the Royal Society such facts only as come immediately under my own observation, and as I think may be worthy of their notice, and leave them at full liberty to reason upon them.

We may flatter ourselves, as a very great progress has been made of late years in the knowledge of volcanoes, that by combining such observations as we are already in possession of, with those which may be made hereafter, in the four quarters of the world (in all of which nature seems to have operated in a like manner), a much better theory of the earth may be established than the miserable ones that have hitherto appeared.

Those who have not had an opportunity of examining a volcanic country, as I have for more than twenty years, would little suspect, that many curious productions and combinations of lava's and tuffa's were of a volcanic origin; especially when they have undergone various chemical operations of nature, some of which, as I have mentioned in a former

communication, as well as in this, have been capable of converting tuffa's, lava's, and pumice stone, into the purest clay.

I have remarked, that young observers in this branch of natural history are but too apt to fall into the dangerous error of limiting the order of nature to their confined ideas: for example, should they suspect a mountain to have been a volcano, they immediately climb to its summit to seek for the crater, and if they neither find one, or any signs of lava or pumice-stone, directly conclude such a mountain not to be volcanic: whereas, only suppose Mount Etna to have ceased erupting for many ages, and that half of its conical part should have mouldered away by time (which would naturally be the consequence) and the harder parts remain in points, forming an immense circuit of mountains (Etna extending at its basis more than one hundred and fifty miles); such an observer as I have just mentioned would certainly not find a crater on the top of any of these mountains, and his ideas would be too limited to conceive, that this whole range of mountains were only part of what once constituted a complete cone and crater of a volcano. It cannot be too strongly recommended to observers in this, as well as in every other branch of natural history, not to be over-hasty in their decisions, nor to attribute every production they meet with to a single operation of nature, when perhaps it has undergone various, of which I have given examples in the island which has been the principal subject of this letter. That which was one day in a calcareous state, and formed by an insect in the sea, becomes vitrified in another, by the action of the volcanic fire, and the addition of some natural ingredients, such as sea salts and weeds, and is again transformed to a pure clay by another curious process

of nature. The naturalist may indeed decide as to the present quality of any natural production; but it would be presumption in him to decide as to its former states. As far as I can judge in this curious country, active nature seems to be constantly employed in composing, decomposing, and recomposing; but surely for all-wise and benevolent purposes, though on a scale perhaps much too great and extensive for our weak and limited comprehension.

I have the honour to be, with great regard and esteem, &c.

W. HAMILTON.

P O S T S C R I P T.

THE earth is not yet so perfectly quiet in Calabria and at Messina, as to encourage the inhabitants to begin to rebuild their houses, and they continue to live in wooden barracks. There has, however, been no earthquake of consequence during these last three months. My conjecture, that the volcanic matter (which was supposed to have occasioned the late earthquakes) had vented itself at the bottom of the sea between Calabria and Sicily, seems to have been verified; for the pilot of one of his Sicilian Majesty's sciabecques, having some time after the earthquakes cast anchor off the point of Palizzi, where he had often anchored in twenty-five fathom water, found no bottom till he came to sixty-five fathom, and having founded for two miles out at sea towards the point of Spartivento in Calabria, he still found the same considerable alteration in the depth of the sea. The inhabitants of Palizzi likewise declare, that during the great earthquake of the 5th of February, 1783, the sea had frothed and boiled up tremendously off their point.

E X P L A -

EXPLANATION OF THE PLATES.

Tab. X. Plan of the island of Ponza.

Tab. XI. View of part of the inside of the harbour of the island of Ponza.

Fig. 1. Rock of lava, which in many parts is formed into regular small basaltic of a reddish cast, having probably been tinged with some ochre. Most of the detached rocks of this island resemble this.

Fig. 2. See p. 374.

Tab. XII. View taken from the outside of the harbour of the island of Ponza, near the Lighthouse.

Fig. 1. Rock of volcanic matter converted to pure clay.

- 2. Ditto, with strata of pumice-stone.
- 3. Rocks of lava inclining to take basaltic forms.
- 4. Rock composed of spherical basaltic.

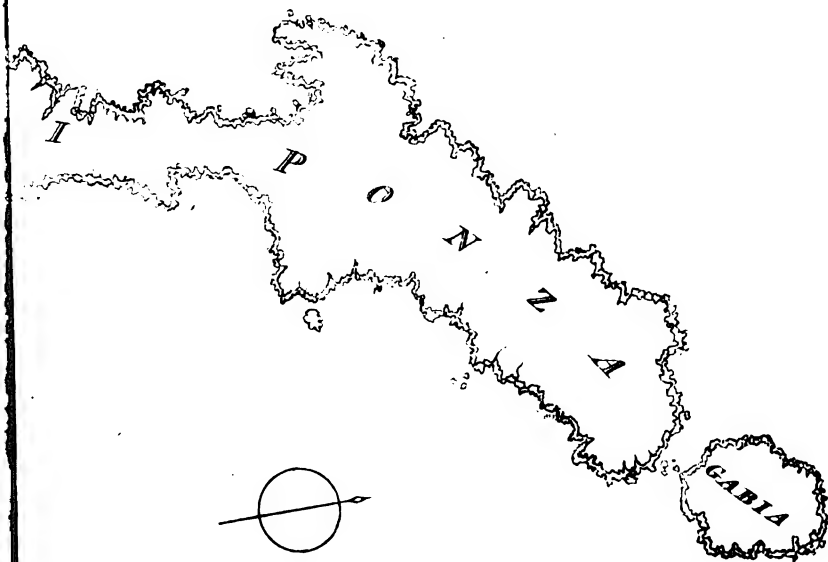
XIX. *An Account of a new Electrical Fish. In a Letter from Lieutenant William Paterfon to Sir Joseph Banks, Bart. P. R. S.*

Read May 11, 1786.

S I R,

WHILE at the island of Johanna, one of the Comora islands, in my way to the East-Indies, with the 98th Regiment, I met with an electrical fish, which has hitherto escaped the observation of naturalists, and seems in many respects to differ from the electrical fishes already described; which induces me to send you the following account of it, with a very imperfect Drawing, and to beg that, if you think it deserves attention, you will do me the honour of presenting it to the Royal Society. The situation of a subaltern officer, in an army upon foreign service, will, I hope, sufficiently apologize for my sending you so very imperfect a sketch of the fish, which was made in the field, in a hot climate, under every disadvantage.

The fish is seven inches long, two inches and a half broad, has a long projecting mouth, and seems to be of the genus Tetrodon. The back of the fish is a dark brown colour, the belly part of sea-green, the sides yellow, and the fins and tail of a sandy green. The body is interspersed with red, green, and white spots, the white ones particularly bright; the eyes large, the iris red, its outer edge tinged with yellow. (See Tab. XIII.)



Engraved by

The island of Johanna is situated in latitude $12^{\circ} 13'$ south. The coast is wholly composed of coral rocks, which are in many places hollowed by the sea. In these cavities I found several of the electrical fishes. The water is about 56° or 60° of heat of FAHRENHEIT's thermometer. I caught two of them in a linen bag, closed up at one end, and open at the other. In attempting to take one of them in my hand, it gave me so severe an electrical shock, that I was obliged to quit my hold. I however secured them both in the linen bag, and carried them to the camp, which was about two miles distant. Upon my arrival there, one of them was found to be dead, and the other in a very weak state, which made me anxious to prove, by the evidence of others, that it possessed the powers of electricity, while it was yet alive. I had it put into a tub of water, and desired the Surgeon of the regiment to lay hold of it between his hands; upon doing which he received an evident electrical stroke. Afterwards the Adjutant touched it with his finger upon the back, and felt a very slight shock, but sufficiently strong to ascertain the fact.

After so very imperfect an account, I will not trouble you with any observations of my own upon this singular fish; but beg you will consider this only as a direction to others who may hereafter visit that island, and from their situation, and knowledge in natural history, may be better able to describe the fish, and give an account of its electrical organs.

I have the honour to be, with great esteem, &c.

W. PATERSON,
Lieutenant 98th regiment.



XX. *Observation of the Transit of Mercury over the Sun's Disc, made at Louvain, in the Netherlands, May 3, 1786. By Nathaniel Pigott, Esq. F. R. S.*

Read June 15, 1786.

Louvain, May 15, 1786.

THE transit of Mercury was to happen a few days after my arrival at this place from England. Although I brought no astronomical instruments with me, I wished to observe this phænomenon; and upon application to M. THYSBAERT, *Président du Collège Royal*, a very distinguished Member of this University, he supplied me, in the politest manner, with the following instruments, and a convenient place for the observation. He carried his attention to the most trifling circumstances, in order to make my situation, in every respect, agreeable. The instruments he provided me with were a Gregorian reflector of 21 inches focal length, with an aperture of 4½ inches, the magnifying power of which I esteemed about 70 or 80, with a good quadrant 18 inches radius, and a compound pendulum clock, steadily fixed, beating dead seconds. These instruments were made in London, and used for the observation

Mr. N. PIGOTT's *Observation of the Transit of Mercury.* 385
 observation of Mercury. The rate of the clock, and the apparent times thence deduced, were obtained by equal altitudes of the sun, taken with the quadrant. These were the only instruments I had, and therefore such observations as are not dependent on the *measure of time*, are to be considered as made by estimation; however, the most important, the internal and external contacts of Mercury, and hence the egress of his center and the interval of time between the two contacts, were made in a very satisfactory manner. About six o'clock, when I attended for the observation, there being a great number of solar spots, Mercury might easily have been mistaken for one; but his motion soon removed every doubt in that respect. Flying clouds obscured the sun at intervals; but during the last half hour, the weather was fine, the sky clear, the limb of the sun well defined; Mercury round and very black. There seems to have been some mistake, in respect of this phænomenon, either in the calculation or the printing of the *Connoissance des Temps* of this year: the emerſion of the center of Mercury is there ſet down at 19 h. 45' apparent time at Paris; whereas, by my observation, the egress of the center at Louvain was at 20 h. 47' 28" or 29" apparent time. Taking here no other equation into conſideration, except the difference of meridians between Paris and Louvain, which, by a great number of observations, I determined in 1775 to be 9' 37" in time *, the emerſion of the center at *Paris* muſt have been at 20 h. 37' 51" or 52", which differs nearly 53' from the computed time. By the ſame reaſoning, I ſhould ſuppoſe, that the emerſion of the center of Mercury at Greenwich was obſerved at 20 h. 28' 35" or 36". Mercury being ſo very near

* See Philoſophical Tranſactions, vol. LXVIII. p. 654.

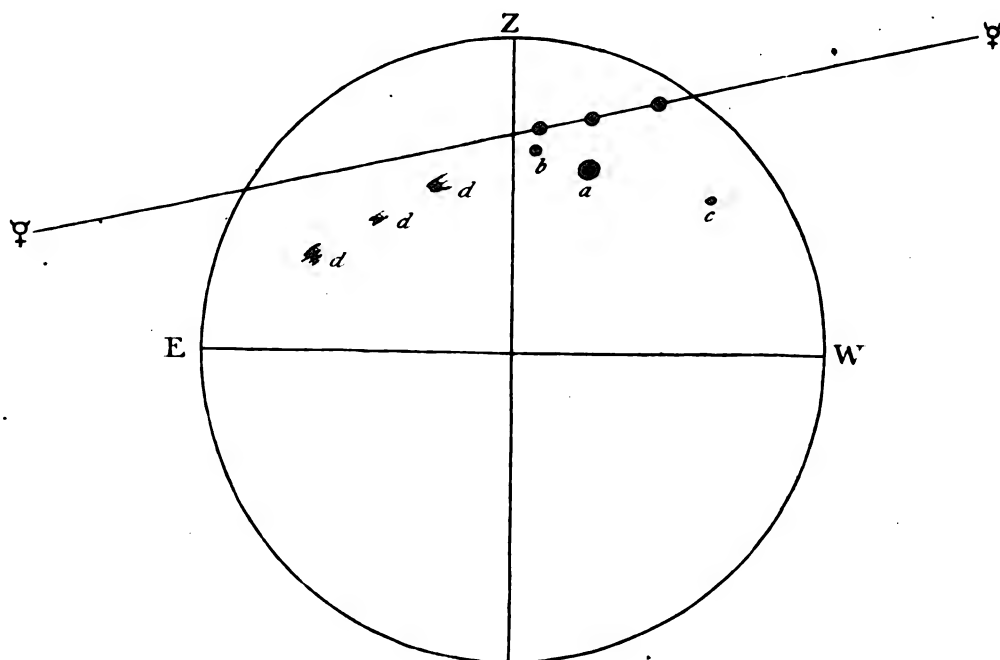
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the earth, the effects of parallax must be considerable, and the western situation of Greenwich of $18' 53''$ in time from this place, must occasion a retardation, which, on computation, may be hereafter allowed for, and added to the supposed time of the egress above-mentioned, deduced from my observation here.

While I am writing this Paper, the respective situation of Greenwich and Louvain strikes me. The latitude of Greenwich is $51^{\circ} 28' 40''$, that of Louvain $50^{\circ} 53' 3''$ *; the difference little more than half a degree. Greenwich is $9' 16''$ west, and Louvain $9' 37''$ east of the Paris Observatory; the parallax above-mentioned is therefore nearly, but in a contrary sense, equal at the two places, and thus the effects of both are compensated relatively to Paris. What other advantage may result from this circumstance, would require consideration. I have not leisure, at present, to revolve it in my mind, as I am desirous to lay this Paper before the Royal Society as soon as I can, by the favour of Dr. MASKELYNE, our Astronomer Royal.

* See Philosophical Transactions, vol. LXVIII. p. 643.

Observations of the Transit of Mercury at Louvain.



Apparent time.

H. M. S.

18 32 30 flying clouds; Mercury ill defined, with some twinkling.

19 13 30 the spot (a) appears thrice as large as Mercury; spot (b) twice ditto.

19 16 30 a perpendicular from the sun's limb on E. W. bisects Mercury and (b).

19 27 30 perpendicular, as above, equi-distant from (a) and (b).

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E e o

Apparent

Apparent time.

H. M. S.

- 19 34 30 perpendicular, as above, bisects Mercury and spot (*a*).
 19 42 30 perpendicular from Mercury on E. W. is beyond
 spot (*a*).
 19 45 30 it is sensibly beyond spot (*a*).
 20 12 30 perpendicular from the sun's limb on E. W. equi-
 distant from (*a*) and (*c*).
 20 27 30 Mercury very black, round and well defined.
 20 45 41 internal contact; perhaps a few seconds too soon.
 20 47 26 emersion of center by *estimation*.
 20 49 16 external contact.
 20 49 41 Mercury certainly clear of the sun.

a. b. c. d. d. d. are spots in the sun; Z. zenith;
 E. East; W. west of the solar disc.

The internal contact being at 20 h. 45' 41'', and the external at 20 h. 49' 16'', the emersion of the center of Mercury must have been at 20 h. 47' 28''½; which differs only 2½ seconds from the estimated time; and the duration of total egress was 3 m. 35 f.

N. B. The reasons why the nine first observations are all marked at 30'' is, that in reality they were set down at the *minute* only; and that I have added 2' 30'' to each to reduce the time by the clock to apparent time; more nicety would have been superfluous: but the *four last* were rigorously computed.



XXI. *Observation of the late Transit of Mercury over the Sun, observed by Edward Pigott, Esq. at Louvain in the Netherlands; communicated by him in a Letter to Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read June 15, 1786.

S I R,

Louvain, May 27, 1786.

WE have been fortunate here in seeing Mercury's egress.
I observed it thus :

Apparent time, May 3.

H. M. S.

20 45 25 Mercury's limb in contact with the sun's limb;
uncertain.

20 45 37 ditto ditto; certain.

20 47 17 Mercury bisected by the sun's limb.

20 49 22 { Mercury quite out; clouds for a short interval,
which renders the observation rather doubtful.

Though the air was not perfectly free from thin clouds, nevertheless the limbs were well defined. At 20 h. 45' 25'', when I first judged Mercury's limb in contact with the sun's, his form, I think, became rather oval. These observations were made with RAMSDEN's two-feet achromatic, magnifying about 70 times. The above times disagreeing so considerably with the tables will, I imagine, not a little surprise M. DE LA LANDE.

I remain, Sir, with great regard, &c.

EDW. PIGOTT.



XXII. *Additional Observations on making a Thermometer for measuring the higher Degrees of Heat. By Mr. Josiah Wedgwood, F. R. S. and Potter to Her Majesty.*

Read June 22, 1786.

MY thermometer for measuring the higher degrees of heat having been honoured with the notice of this illustrious Society, I now request a further indulgence for a few more observations on the same subject.

In my first Paper * I communicated every thing that experience had then taught me, respecting both the construction and use of this thermometer; but more extensive practice has since convinced me, that other managements and precautions are necessary, in order to bring it to the perfection it is capable of receiving: for pieces made of the same clay, and exactly of the same dimensions, have been found to differ in the degree of their diminution by fire, in consequence of some circumstances in the mode of their formation, at that time unheeded, and very difficult to be developed.

Of the two ways proposed for forming them, the mould and the press, the former was made choice of, as being, for general use, the most commodious. The soft clay was pressed into a square mould with the fingers; and the pieces, when dry, were pared down on two opposite sides, by means of a

paring gage made for that purpose, so as to pass exactly to 0° at the entrance of the converging canal of the measuring gage.

But the pieces thus formed have been found liable, in passing through strong fire, to receive a little alteration in their figure, which produces an uncertainty with respect to their subsequent measurement: the two sides, instead of continuing flat, become concave; the edges, both at top and bottom, projecting beyond the middle part, sometimes very considerably, as at *a* and *b*, fig. 1, (Tab. XIV.) where AB represents a perpendicular section of an unburnt piece, and *ab* a like section of the same piece after it has undergone a heat of 160 degrees. This irregularity in the form, which is sensible only after passing through the high degrees of fire, was observed in some of the early experiments, but was not then looked upon as being productive of any error.

On more attentively examining this matter, it appeared, that when the clay is pressed into a mould, the surface in contact with the mould acquires a more compact texture than the inner part of the mass;—that this compactness restrains, in some degree, its diminution in the fire;—and therefore, that when this surface, or less diminishable crust, is pared off, from the two sides only, the piece may be considered as having its upper and lower strata (AA and BB, fig. 1.) composed of a less diminishable matter than the intermediate part, the necessary consequence of which structure will be such a figure as we find the pieces to assume; for if any stratum in the mass shrinks less than the rest, the extremities of that stratum must be left proportionably prominent. That this was the true cause of the inequality, I was convinced by firing some pieces *unadjusted*, with all their surfaces entire, as they came from the mould; for these pieces, after passing through the same strong

fires

fires with the preceding, continued flat, with the angles regularly sharp, and without the least sensible prominence in any part.

Some of the moulds, employed for this use, were made of plaster, a material more convenient for the workman than metal, as the pieces part more freely from it, but which contributed greatly to increase the above-mentioned irregularity: for the plaster, by absorbing a portion of the water from the clay contiguous to it, renders the surface at the same time, even at the instant of contact, much more *consistent*, and consequently more difficult to press into the angles of the mould; so that the outsides of these pieces were not only more *compressed*, but formed of clay of a different *temper* from the inner parts, being much drier or firmer; a circumstance which, as will appear hereafter, restrains still more their diminution in the fire.

The moulds were therefore laid aside, and the press adopted in their stead; for as the soft clay, pressed in a cylindrical vessel, gives way and escapes through an aperture made for that purpose (by which means it is formed into long rods), the sides of the piece cannot be supposed to receive so great a degree of compressure against the sides of the aperture through which it is *delivered* in this operation, as it does against the sides of the mould, by which it is *confined* till every part has born a pressure sufficient to force the clay into every angle, which is much greater than even a workman would imagine till he comes to try the experiment himself.

But with this change some new difficulties arose; for pieces pressed through the same aperture, and from the same pressful of clay, and adjusted, when dry, to the same point in the gage, were found, after passing together through the same
strong

strong fires, to differ in their dimensions from one another, in some instances more than any of the preceding.

Having hitherto paid no particular attention myself to the mere manual labour of pressing the clay, I determined, upon this event, to go through that and every other operation, however simple and seemingly insignificant, with my own hands. In doing this I observed, that the power necessary for forcing the clay through an aperture which bore but a small proportion to the diameter of the mass of clay in the press, was so great as to squeeze out, along with the clay that first passed through, a considerable portion of the water that belonged to the rest. From this over-proportion of water in the composition of the first pieces they were soft and spongy, and the succeeding ones more and more compact, till at length the clay proved so stiff as scarcely to be forced through at all.

Clay, containing different proportions of water, is well known to diminish differently in drying; but it was not imagined that, when dry, there would be any difference in its subsequent diminutions by fire. Experiments however, multiplied in a variety of circumstances, shewed decisively, what the pieces formed in the mould had given grounds to suspect, that those formed of the softest clay, and which had undergone the least pressure, diminished most in burning; and that the diminution is uniformly less and less, in proportion to the greater degree of pressure or compactness.

The knowledge of the cause of the irregularity suggested a remedy. I lessened the width of the press very much, so as to bring the diameter of the mass of clay, and that of the aperture through which it is delivered, to a nearer proportion with one another. A much less degree of force being now sufficient, the pieces, or rods, were proportionably more uniform, though

though there was still a sensible difference, in consistence, between those which were first and last pressed out from the same mass of clay. The intermediate ones, within a certain distance from the two extremes, corresponded very nearly with one another; so that by rejecting a sufficient number of the first and last, and using the *intermediate ones only*, the inequality may be considered as almost annihilated.

I nevertheless still found that, in strong fire, the edges became a little prominent, though not so much as before. I was aware that these pieces must partake, in some degree, of the imperfection of those made in the mould; having their surfaces rendered, by their friction against the sides of the aperture, more compact than the inner part. But I suspected that something might depend also upon the *form*, and accordingly made many trials for ascertaining the form that might be least liable to this irregularity: the angles only were bevilled off, the sides were rounded, the pieces were rounded all over, made of ovals and other curves, and both the longest and shortest dimensions were used as the extent to be measured: the general result was, that the nearer they came to a circular figure, the less inequality they contracted in the fire, and by making them entirely circular, the imperfection appeared to be obviated altogether; cylindric pieces bearing the strongest fires without the least appearance of prominence or inequality in any part of their surface. I have therefore chosen this last form, leaving only one narrow flat side (*ab*, fig. 2.) as a bottom for the pieces to rest upon, and to distinguish the position in which they are to be measured in the gage.

I have endeavoured at the same time to obviate whatever inaccuracy the inequality of compactness may be capable of producing, by so adjusting the aperture through which the rods

rods are pressed, and on which their figure and dimensions depend, as to supersede the use of the paring gage altogether; that the whole surface may remain of the same uniform compactness which it received in the press. And as it is scarcely practicable, in any mode of forming soft clay, to have all the pieces precisely of the same dimensions after drying, I do not reject those which come within two or three degrees of the standard, but, instead of injuring the surface by paring or rubbing, I mark on the ends the degrees which they respectively exceed or fall short; which degrees are accordingly to be subtracted, or added, in all observations of heat made with those pieces. Strictly speaking, an allowance ought to be made also for the proportional diminution upon this excess or deficiency itself; but the allowance for three degrees would not, at the melting heats of copper, silver, or gold, amount to more than a seventh part of a degree; and at the extreme point of heat that I have been able to attain, when the piece has diminished $\frac{1}{4}$, or nearly one-fourth of its whole thickness, it comes only to four-fifths of a degree.

It may be proper to take notice of an irregularity in the *apparent* diminutions of the pieces, which was sometimes observed to happen from another cause, their bending a little in strong fire, so as to be prevented from going so far in the gage as they would have done if they had continued perfectly straight. But as this takes place only in pieces of considerable length, and as they derive no advantage of any kind from that length, the remedy is too obvious to need being here mentioned.

Another fallacious appearance arose, not from any imperfection in the pieces themselves, but from a deception with respect to the heat in which the comparison of them had been

made. In one period of the course of my experiments, I employed, for firing them, a small, shallow, cylindrical vessel (fig. 3.) setting the pieces on end, close together, on its bottom, and placing it in the middle of the fuel, in a common air-furnace, with care to keep the fire as equal all round it as possible. It was expected, that all the pieces would receive an equal heat; and as they were found, after the operation, to differ in their dimensions, sometimes considerably, from one another, these differences proved a source of much perplexity, till it was discovered that the pieces had really undergone unequal degrees of heat.

In the paper on the comparison of this thermometer with FAHRENHEIT's*, I have taken notice of the great difficulty of obtaining, in small furnaces, a perfectly equal heat, even through the extent occupied by a few of these little pieces: and how different the heat may be in different parts of one vessel, we may be satisfied by an easy experiment, *viz.* setting a cylindrical rod of clay, of the length of eight or ten inches, upright in the middle of a crucible, and urging it with strong fire in a common small furnace; the rod will be found very differently diminished at different parts of its height; and if its height be sufficient to reach some way above the fuel, nearly the whole range of the thermometric scale may be produced in one rod; an ocular proof, not only of the diversity of heat within a small compass, but likewise of the *peculiar* sensibility of this thermometer, every *part* of the mass expressing distinctly the degree of heat which it has itself felt. It will be proper, in this experiment, to have a tube fixed in the bottom of the crucible, for keeping the rod steady, as at fig. 4. By this means the heat of my air-furnace renders a

* Philosophical Transactions, vol. LXXIV.

rod of the thermometric clay tapering, from about four parts in diameter at top to three at bottom, which are nearly the proportions between the width of the piece when unburnt, or but just ignited, and when it has suffered a heat of 160 degrees.

To the foregoing sources of inequality in the pieces, one more may be added, small cavities, or air-bubbles accidentally inclosed, which sometimes happened in the earlier experiments. In order to prevent these, particular attention is now paid by the workmen to what we call *banding* or *slapping* the clay, an operation by which its different parts are intermixed, and the mass rendered of an uniform temper throughout. The workman takes a lump of the clay in his hands, perhaps of two pounds weight, and, breaking it in two in the middle, lays one part upon the other, and presses them flat again, repeating this forty or fifty times, or perhaps oftener. Now, considering the pieces at first as two dissimilar masses, with any number of air-bubbles inclosed; each of these pieces being by the first doubling divided into two, by the next into four, by the third into eight, and so on in geometrical progression, each of the original masses will be divided by the fiftieth repetition into upwards of eleven thousand millions of millions of invisible laminæ:—invisible, because the lump of clay would, long before the last doubling, be of one uniform colour, though at first one-half of it had been black, and the other white. If therefore no air be inclosed between the pieces at the times of their being put together in this process, all the air which might have been in the mass before would certainly be driven out; and, to avoid as much as possible the introduction of any fresh portions of air, the two separated pieces are each time made smooth, and a little convex, on the surfaces that are to be brought together.

By due attention to the circumstances above stated, any single quantity of clay may be made up into thermometer-pieces, that shall differ very little, if any thing at all, from one another.

But a new difficulty now arose, more embarrassing than any of the former; that of procuring fresh supplies of clay, of the same thermometric quality with the first. The quantity of the clay which, after trial of many others, I had made choice of, was small; but the particular spot it was taken from being known, and having purchased the little estate in which it was raised, I had not a doubt of obtaining more of the same when it should be wanted: for clays in general, when raised from an equal depth, in the same stratum, and near the same place, are found to possess the same properties, with respect to ductility in the hands of the workman, a disposition to assume by fire a porcelain or vitreous texture, singly or in composition, and all other qualities relative to their use in pottery. In this, however, I was deceived; for when a fresh supply was wanted, to complete my experiments, though I had some taken from a pit joining to the first, and at the same depth, it was found to diminish differently from the former parcel. I then had some raised from different parts of the same field and bed, and at different depths; and in various other places in Cornwall, from the spot where this species of clay is first met with to the Land's-End; but all these clays differed so much from the first in the quantity of their diminution by fire, and most of them likewise from each other, that I despaired of being ever able to find one that would correspond with it, or any natural clays that could be obtained twice of exactly the same *thermometric* properties, how similar soever in other respects.

Upon a review of the numerous comparisons which I have made of these new clays, in different degrees of heat, from the

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commencement of redness up to intense fire, the most striking differences of the greatest part of them from the old seemed to originate in the lower stages of heat; and of those which were got from the neighbourhood of the old, the variations from it in the higher stages seemed, for the most part, to be only consequences of those differences in the lower ones.

I have mentioned, in the first Paper, that the original thermometer-pieces had their bulk enlarged a little on the approach of ignition; but that by the time they became visibly red-hot throughout, they are reduced to their former dimensions again; and at this moment the thermometric diminution begins. The new clays had their bulk enlarged in a much greater proportion, and the enlargement was of much longer continuance: some of them required a heat of 15 degrees to destroy the increase which ignition had produced in their bulk, and bring them back to their original dimensions: after this period, most of them diminished pretty regularly, and uniformly with the old, being nearly so many degrees behind it, in all the succeeding stages of heat, as they required to bring them back from the enlarged state.

I have mentioned also, in my former paper, that a quantity of air is extricated from the clay, most rapidly at the period in which the augmentation of bulk takes place; and that the augmentation was probably owing to this air forcing the particles of the clay a little asunder, previous to the instant of its escape. It was therefore presumed, that the greater extension of these new clays might be owing, either to a greater quantity, or stronger adhesion, of this combined air: and as clay, kept moist for a length of time, in certain circumstances, undergoes a process seemingly analogous to fermentation,

tion, it was hoped that, by such a process, part of its combined air might be detached.

But experiments made on this idea have proved, that these clays, kept moist for a twelvemonth,—kept for a considerable length of time in a heat just below visible redness,—boiled in water for many hours,—alternately, and repeatedly, moistened and dried,—suffer no alteration in their thermometric properties, and continue to differ from the standard clay just as much as they did at first.

Some of these new clays differed from the old in a property still more essential, and by which I was much more disconcerted; for though they continued diminishing with tolerable regularity, keeping only some degrees behind it, up to a certain period of heat, about that in which cast iron melts; yet many of the pieces, urged with a heat known to be greater than that, were found not to be diminished so much as those which had suffered only that lower heat. Further experiments shewed, that, after diminishing to a certain point, they begin, upon an increase of the heat beyond that point, to swell again: and as this effect is constant in certain clays, and begins earliest in those which are most vitrescible, and as clays are found to swell upon the approach of vitrification, I look upon this second enlargement of bulk, however inconsiderable, as a sure indication of the clay or composition having gone beyond the true porcelain state, and of a disposition taking place towards vitrification; which stage is always, so far as my experience reaches, attended with a new extrication of air; and in some instances, this air being unable to make its escape from the tenacious mass that envelopes it, the burnt clay is thereby so much increased in bulk as to swim on water like very light wood. The degree of heat therefore, at which this enlargement

ment begins, may be considered as a criterion of the degree of vitrescibility of the composition; which points out a new use of this thermometer, enabling us to ascertain the *degree of vitrescibility* of bodies that cannot actually be vitrified by any fires which our furnaces are capable of producing.

All my researches among the natural clays proving fruitless, and the experiments having shewn that all those, which could sufficiently resist vitrification, diminished *too little* in the fire, I endeavoured to find a body possessed of the opposite property, that is, diminishing *too much*, and, by a mixture of these two, to produce the *medium* diminution required. As I could not find any natural substance possessed of that property, which would not at the same time render the compound too vitrescible, I was obliged to have recourse to some artificial preparation; and as the earth of alum is the pure argillaceous earth, to which all clays owe their property of diminution in the fire, possessing that property in a greater or less degree according to the quantity of alum earth in their composition, I mixed some of this earth with the clay, and found it to answer my wishes completely, both in procuring the necessary degree of diminution, and increasing its unvitrescibility. So little is this compound disposed to vitrification, that the greatest heat I could give it, that of 160° , did not even bring it to a porcelain texture, but left it still bibulous; and as it does not arrive at the *porcelain* state in this fire, there can be no danger of its approaching too near to the *vitrescent* in any heat that we can produce in a furnace.

In order to obtain the exact medium required, I took one of the best of the clays I had procured from Cornwall, and mixed it with different proportions of the alum earth, till the composition was found, on repeated trials, to agree with the original

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in all degrees of heat. This coincidence was not indeed essential; but as many degrees of heat were already before the public, measured by thermometer-pieces made of the first clay, and as the correspondence of the first with FAHRENHEIT's scale had likewise been in some measure ascertained, it was desirable that the same degrees of heat should continue to be expressed by the same numbers.

The alum earth is prepared for this purpose by dissolving the alum in water, precipitating with a solution of fixed alkali, and washing the earth repeatedly with large quantities of boiling water: when the earth has settled, the water above it is let off by cocks in the sides of the hogheads; and when the vessels are filled up with fresh water, care is taken to stir up the earth from the bottom, and mix it thoroughly with the liquor. I find it most convenient to use the earth undried, in its gelatinous state, as in this state it unites easily and perfectly with the clay; whereas, when the alum earth has concreted into dry masses, great labour is necessary to mix them uniformly together.

I have tried several different parcels of English alum, from the same and from different manufactories, and found no material difference in the quantity of earth it contains*. Nor indeed would it be of any consequence if there was a difference in this respect, as the proportion of alum earth necessary for

* A difference in the quantity of earth *may* arise from different proportions of GLAUBER's salt and vitriolated tartar, of which I have found quantities very considerable, but nearly alike, in all the English alum I have examined. These salts are doubtless formed by the kelp ashes employed in the preparation of the alum. They are discovered by calcining the dried alum with charcoal powder, which decomposes the alum only, leaving the other two salts intermixed with the alum earth, from whence they may be extracted by water.

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different clays, and even for different parcels of the same clay, can only be ascertained by repeated trials, adding successive quantities of the earth till the desired effect is found to be produced. Ten hundred weight of the Cornwall porcelain clay which I have now in use required all the earth that was afforded by five hundred weight of alum.

It is material in this place to observe, that the earth of alum is extremely tenacious of water, insomuch that, though apparently dry, the water and air amount to near as much as the pure earth, and are not to be completely driven out without a full red heat. When divided by the admixture of other earthy bodies, it parts with its water easier indeed than before; but a mixture containing so much of it as the thermometric composition does, is far more retentive of water than common clay, and requires to be kept for some time in a heat equal to that of boiling water, before it is to be considered as dry, that is, before the adjustment of the pieces in the gage. If they are adjusted when only apparently dry, or of such a degree of dryness as they can be brought to by a heat that the hand can bear, the heat of boiling water will diminish them two or three degrees; and the greatest part of what they have thus been deprived of, they gradually recover again on being exposed to the atmosphere, so that the adjustment must be made immediately after the boiling heat.

By the same expedient to which I have thus been obliged to have recourse for procuring to the porcelain clay of Cornwall the standard degree of diminution, and resistance to fire, the same qualities may probably be communicated to any other clay that is tolerably pure from calcareous earth and iron; so that the thermometer clay is no longer to be considered as the produce of any particular spot (which was the principal incon-

venience originally imagined to attend it), but may be procured and prepared in all parts of the world where good common clay, and alum, are to be found; and *corresponding* thermometers may, consequently, be constructed, without any standard to copy from. For, if a converging canal be formed, of any convenient length, with the widths at the two ends in the proportion of 5 to 3, with the sides perfectly straight, and divided into 240 equal parts, numbering the divisions from the wider end*;—and if a clay be obtained of such quality, that when formed, in the manner already mentioned, into pieces of such size as to enter to 0 in the gage or canal, these pieces shall just begin to diminish, or go a little further in the canal, by a heat visibly red;—go to 27, by the heat in which copper melts;—about 90 by the welding heat of iron; about 160, by the greatest heat that can be produced with coaked pit-coal in a well constructed common air-furnace, about eight inches square, still continuing bibulous, so as to stick to the tongue: such gages, and pieces of such clay, so adjusted, will always compose correspondent thermometers.

Having mentioned occasionally several alternate periods of dilatation and contraction in clay, it may be proper to state, and bring into one view, the whole succession of changes which I have observed in this curious material; as otherwise they might create some confusion in the minds of those who have not had occasion to think attentively on this subject, and lead them to ask how a body so variable, and liable to such opposite changes from different degrees of heat, can yet be a just measure of those degrees.

* Or the divisions on the side may be continued to 300; and in that case, instead of the widths of the two ends being in proportion of the odd numbers 5 and 3, the one will be just double to the other.

The changes which take place in all the natural clays that have come under my examination are six.

1. The first is, the *shrinking* of the moist clay in drying, from the mere loss of its water. The purer the clay is, the more water it requires to soften it, and the more it diminishes in bulk by the loss of that water.

2. The dry clay, gradually heated, preserves its bulk unvaried up to the approach of ignition. At this period it is *enlarged* a little; probably, as already observed, from its combined air endeavouring to escape.

3. When this air has made its escape, the clay begins to diminish, or to *lose the bulk it had before acquired*; and returns back, sooner or later, to the same dimensions which it was of when dry. It is at this point that the thermometric diminution commences.

4. From this point the clay continues to *diminish* more and more in proportion as the heat is increased. This I call the *thermometric stage* of diminution: it is of greater or less extent, terminating at different periods of heat, according to the nature of the clay: in the standard thermometer clay, it commences with visible ignition, and continues to (doubtless far beyond) the extreme heats of our furnaces, an interval consisting of 160 degrees of the scale: in others, it begins 4, 6, and in some even 15 of those degrees later, and terminates also much sooner: and in some its whole extent is not above 20 of the same degrees. Throughout the greatest part of this stage, the clays are found to retain their property of sticking to the tongue and imbibing water: between this *bibulous* state and the *vitrescent* there is an intermediate one, distinguished by the name of *porcelain*; and to the higher term of this porcelain state the stage of thermometric diminution seems to continue.

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5. When the clay has passed the porcelain state, it begins to be *enlarged* again, a symptom of the vitrescent stage being commenced; and in this period it swells more or less, according to the nature of its composition.

6. By further heat the swelled mass, becoming fluid, subsides, is converted into glass or slag, and *contracted into less volume* than the clay occupied in any of its preceding states.

It is plain, therefore, that clay can be a measure of heat no further than from ignition, or that point beyond ignition where the third stage terminates, to the beginning of the vitrescent stage; and that, as the three first changes are completely passed before the clay is applied to thermometric purposes, being strictly no other than preparatory processes, the thermometer-pieces, whatever *clay* they may be made of (provided it is sufficiently unvitrescible), are to be considered as possessing only the fourth stage. But a singular property of the *composition* of clay and alum earth remains to be mentioned, *viz.* that it has really no other than this one stage: it suffers no enlargement of its bulk at ignition, or in any other period; but proceeds in one uninterrupted course of diminution, from the soft state in which the pieces are formed, up to the extreme fires of our furnaces. Though the diminution, however, is uninterrupted, it is at the same time so inconsiderable at the beginning, from the heat of boiling water (at which the pieces are adjusted) up to ignition, that the same point of visible redness is taken for the commencement of the scale, in this as in the original clay, without any sensible error or variation in their progress.

I am inclined to believe, though experiments have not yet enabled me to speak with certainty on this point, that the same cause which enlarges the *natural clays* on their first exposure to the fire, operates also in this *composition*, but in a much lower degree;

degree; that while the natural *clays* have their whole mass distended by the efforts of the air in forcing its passage, the *composition* is only restrained in its diminution, or prevented from diminishing so fast as it otherwise would do, and as it is found to do in the subsequent part of its course, after the air has escaped from it.

As the composition of clay and alum earth is far more tenacious of water than the clay itself, and was found, after being dried by the heat of boiling water, to yield, by distillation in a retort, above three times as much aqueous fluid as the original thermometric clay did; it seems probable, that a part of this water, retained to the approach of ignition, and in a state of chemical combination, may facilitate the passage of the air, serving as a vehicle to convey it off through interstices not permeable to air alone, and consequently enabling it to escape without doing that violence to the mass, which the natural clays sustain from the expulsion of their air after the water has been detached from it; for the experiments of Dr. PRIESTLEY have shewn, that vessels even of burnt clay are permeable to air when they have imbibed water into their substance, though not at all so in a dry state.

I have now communicated the result of a series of experiments which have taken considerable time, attention, and labour to complete. Whether the importance of the object will justify me in troubling this illustrious Society with so minute a detail of the most material operations, and their results, is not for me to determine. If the thermometer should not yet be brought to the perfection that may be wished, I flatter myself that some abler hand may now take up the subject to more advantage; and that philosophers and artists will

not

not be less successful in supplying what may still be deficient, and in ascertaining, by the *contraction of argillaceous matter*, the measurements and effects of the various degrees through the immense extent of luminous fire, than they have been with respect to the limited and narrow compass of low heat, which is measurable by the *expansion of fluids*.

Fig. 1.

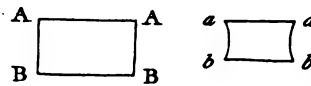


Fig. 2.

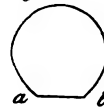
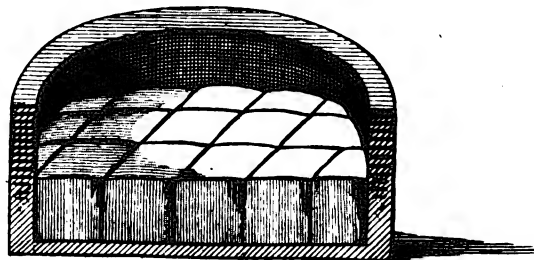


Fig. 3.



XXIII. *The Latitude and Longitude of York determined from a Variety of Astronomical Observations; together with a Recommendation of the Method of determining the Longitude of Places by Observations of the Moon's Transit over the Meridian. Contained in a Letter from Edward Pigott, Esq. to Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read June 29, 1786.

S I R,

Bootham, York, March 16, 1786.

THE great number and variety of observations I have made for determining the longitude and latitude of York will, I believe, settle those points very accurately: I therefore wish to have them presented to the Royal Society, and beg the favour of you to be at that trouble. The instruments I used were a good gridiron pendulum clock, a $2\frac{1}{2}$ feet reflector, an eighteen-inch quadrant by BIRD, and a transit instrument made by Sisson.

The difference of meridians between Greenwich and York was found by the following methods.

Occultations

Occultations of stars by the moon.

	App. time.	
1783	h. "	
Sept. 10	11 34 44½	{ York, immersion of a star of the ninth magnitude during the eclipse of the moon; good.
		{ Paris, at L'Observatoire de la Marine; ditto.
	11 49 39½	{ Ditto, by M. MESSIER, who determined its R.A. 349° 22' 17" and south declination 5° 27' 54".
Oct. 7	14 26 28½	{ York, immersion of ϕ Aquarii, instantaneous.
	14 37 15½	{ Greenwich, ditto.
Dec. 30	8 1 24	{ York, immersion of δ Piscium, instantaneous: I find I wrote down the minute wrong, it is here corrected.
	8 2 56½	{ Greenwich, immersion of ditto.

Mr. GOODRICKE was so obliging as to be at the trouble of computing these occultations, and sent me the results as follows:

By the star of the ninth magnitude	-	-	-	4' 29"	} Difference of meridians between Greenwich and York.
By ϕ Aquarii	-	-	-	4 23	
By δ Piscium compared to the Greenwich observation	4' 30"			4 29	
Ditto, to Mr. WOLLASTON's observation,	-	4 28		4 29	
On a mean	-	-	-	4 27	

Observed meridian R.A.'s of the moon's limb.

In 1783 this method of finding the difference of meridians occurred to me, and I wrote to Mr. BAYLEY, your late Assistant, for information, being entirely ignorant it had ever been noticed; but have since seen, Sir, that you mention it in your valuable Instructions for the Observations of the Transit of Venus, annexed to the Nautical Almanac of 1769. I have also just perused on the same subject Abbé TOALDO's ingenious pamphlet written in 1784, which you were so kind as to send

me. Still I find that the great exactness of this method is not suspected; I therefore shall, in the latter part of this Paper, enter into some necessary detail, being convinced that, in a very short time, it must be universally adopted, having every advantage over Jupiter's first satellite, and but little inferior in precision to occultations.

Difference of our meridians by each observation.

1781, Dec. 20.	4' 36"	1784, July 2.	4' 23"
Dec. 29.	4' 10"	Nov. 20.	4' 23"
1782, June 17.	4' 25"	Dec. 20.	4' 27"
Nov. 30.	4' 20"	Dec. 22.	4' 20"
Dec. 18.	4' 25"	1785, Mar. 19.	4' 25"
1783, Nov. 3.	4' 32"	Aug. 16.	4' 22"
Dec. 6.	4' 39"	Aug. 18.	4' 36"
Dec. 30.	4' 16"	Sept. 12.	4' 35"
1784, May 1.	4' 8"	Sept. 17.	4' 25"
May 25.	4' 11"	Nov. 12.	4' 34"
		Nov. 14.	4' 18"

4' 24" $\frac{1}{4}$ on a mean.

Observations of Jupiter's first Satellite.

Dates, &c.	App. time.	
1782, June 3.	h. 12 36 48	York, it immerged near Jupiter.
	12 51 9	Paris, M. MECHAIN.
Immersion.	12 51 7	Paris, M. CASSINI.
	13 57 40	Buda, Father WEISS.

Observations of Jupiter's first Satellite continued.

Dates, &c.	App. time		
	h.	m.	
1782, July 21	9 35	10	York.
Emersions.	9 39	21	Greenwich, Dr. MASKELYNE.
	9 48	54	Paris, M. MECHAIN; high wind.
	9 48	46	Paris, M. CASSINI.
	10 55	15	Buda, Father WEISS; moon very near Jupiter.
1783, July 3	12 9	50	York; it immersed near Jupiter.
Immersions.	12 14	20	Greenwich.
	12 24	8	Paris, M. MECHAIN.
Sept. 17	9 48	15	York.
Emersions.	9 47	44	York, Mr. GOODRICKE; very good.
	9 46	39	Oxford, Mr. HORNSBY.
	10 1	0	Paris, M. MECHAIN; very good.
1784, Aug. 4	10 10	55	York; tolerably good.
Immersions.	10 10	57	York, Mr. GOODRICKE; middling.
	10 24	57	Paris, M. MECHAIN; air a little hazy.
Sept. 3	14 39	52	York; emerged near Jupiter.
Emersions.	14 53	51	Paris; thinks rather too late.
Sept. 5	9 8	54	York; good.
Emersions.	9 13	15	Greenwich, Dr. MASKELYNE.
	9 22	18	Paris, M. MECHAIN; 6 feet reflector, magnifying 450 times.
	9 22	45	Paris; with a $3\frac{1}{2}$ tripl. object glass achromatic.
Sept. 12	11 6	9	York; good.
Emersions.	11 6	24	York, Mr. GOODRICKE; very good.
	11 10	42	Greenwich, Dr. MASKELYNE.
	11 19	47	Paris, M. MECHAIN; as on the 5th.
	11 19	50	Paris, M. MECHAIN; as on the 5th.
1785, July 15	13 37	32	York; good.
Immersions.	13 42	1	{ By tables corrected by the observations of Greenwich and Marseilles of July 31, 1785.
July 31	11 53	18	York; good.
Immersions.	11 57	32	Greenwich; air very clear.
	12 18	53	Marseilles, M. BERNARD.

Obfer-

Observations of Jupiter's first Satellite continued.

Dates, &c.	App. time.	
	h. ' "	
1785, Aug. 30	14 2 59	York; excellent; air remarkably clear.
Immersions.	14 7 3	Greenwich; ditto.
	14 28 33	Marseilles, M. BERNARD.
Sept. 15	12 25 2	York; good.
	12 25 4	York, Mr. GOODRICKE; good; moon-light.
Immersions.	12 29 23	Greenwich; air clear.
	12 50 46	Marseilles, M. BERNARD.
Nov. 18	7 58 6	York.
Emerfions.	8 2 39	Greenwich; air very clear.
	8 12 2	Paris, M. MECHAIN; a thin cloud.
Dec. 2	11 44 24	York; Jupiter rather low.
Emerfions.	11 49 13	Greenwich; ditto; air clear.

By letters from M. MECHAIN, Buda is 1 h. 6' 33" east of Paris, and Marseilles also east o h. 12' 7".

I observed with a 2½ feet reflector, which I believe to be about 10" of time inferior to the telescopes of Greenwich, Oxford, Paris, and Buda. As for Marseilles no instrument is mentioned; therefore, except for that place, 10" must be added to my immersions, and the same subtracted from the emerfions; then the difference of meridians between Greenwich and York will be as follows, when each of the observations is compared to mine, and a mean thereof taken.

Immersion.			Emerfions.		
1782, June 3.	4	54	1782, July 21.	4	29
1783, July 3.	4	36	1782, Sept. 17.	4	8
1784, Aug. 4.	4	36	1784, Sept. 3.	4	53
1785, July 15.	4	19	Sept. 5.	4	33
July 31.	4	8	Sept. 12.	4	37
Aug. 30.	4	3	1785, Nov. 18.	4	46
Sept. 15.	4	16	Dec. 12.	4	59
<hr/>			<hr/>		
4 24½ on a mean			4 38		
<hr/>			<hr/>		

Therefore, by a mean of the immersions and emerfions, York is 4' 31" west of Greenwich. Mr. GOODRICKE's emerfion of Sept. 17, 1783, is used instead of mine, it being undoubtedly more exact.

To enter into any detail concerning the eclipses of Jupiter's fatellites would be useless, as it is a matter so amply considered by every astronomer. I shall only say that the exactness expected even from those of the first fatellite is, in my opinion, too highly rated. Among the various objections, there is one I have often experienced, and which proceeds solely from the disposition of the eye, that of seeing more distinctly at one time than at another. It may not be improper also to mention, that the observation I should have relied on as the best, that of August 30, 1785, marked excellent, and air remarkably clear both at Greenwich and York, is one of those which differ the most from the truth. This I remark without having the most distant inclination of drawing any conclusion; a single instance can be of no weight,

Part of the eclipse of the Moon, Sept. 10, 1783.

The two last columns shew the difference of meridians between Greenwich and York. The observations marked with an asterisk were made by Mr. GOODRICKE.

Spots observed.	York, by Mr. GOODRICKE and me. App. time.	Paris, by M. ME- CHAIN. App. time.	Paris, by M. MES- SIER. App. time.	Diff. of meridians by M. ME- CHAIN.	Diff. of merid. by M. MES- SIER.
	h. ' "	h. ' "	h. ' "		
Galileus bisected -	9 45 32	9 58 33	- - -	3 38	- -
Aristarchus covered -	9 49 13*	10 2 38	- - -	4 2	- -
Copernicus touches {	9 57 3*	10 11 18	10 11 9	4 52	4 48
	9 57 20	10 11 18	10 11 9	4 35	4 31
Copernicus bisected {	9 58 33*	10 12 5	- - -	4 9	- -
	9 58 55	10 12 5	- - -	3 47	- -
Copernicus covered -	9 59 9*	10 12 57	10 12 41	4 25	4 14
Plato touches -	10 5 7	10 18 37	10 18 40	4 7	4 15
Plato covered -	10 6 18	10 19 52	10 19 28	4 11	3 52
Manilius touches - {	10 11 26*	10 25 34	10 25 24	4 45	4 40
	10 11 33	10 25 34	10 25 24	4 38	4 33
Tycho touches - {	10 11 57*	10 25 34	10 25 24	4 14	4 9
	10 11 57	10 25 34	10 25 24	4 14	4 9
Manilius covered -	10 12 47*	10 26 29	10 26 53	4 19	4 48
Tycho covered - {	10 13 8*	10 27 19	10 27 8	4 48	4 42
	10 13 32	10 27 19	10 27 8	4 24	4 18
Menelaus bisected -	10 15 41	10 29 19	- - -	4 15	- -
Prom. Acut. Cen. covered	10 25 26*	10 39 5	- - -	4 16	- -
Proclus bisected -	10 29 00	10 42 28	- - -	4 5	- -
Mare Crisum touches {	10 30 18*	10 43 56	10 44 00	4 15	4 24
	10 30 18	10 43 56	10 44 00	4 15	4 24
Mare Crisum bisected	10 32 43*	10 46 34	10 46 10	4 28	4 9
Mare Crisum covered	10 35 38*	10 49 11	- - -	4 10	- -
Grimaldus emerges -	12 23 30	- - -	12 37 5	- -	4 17
Grimaldus bisected -	12 23 44	12 36 48	- - -	3 41	- -
Grimaldus emerged {	12 23 55*	12 37 25	- - -	4 7	- -
	12 23 59	12 37 25	- - -	4 3	- -
Galileus emerges -	12 25 50*	- - -	12 39 1	- -	3 53
Galileus bisected -	12 25 56*	12 39 16	- - -	3 57	- -
Aristarchus bisected.	12 28 59	12 43 8	- - -	4 46	- -

Difference of meridians on a mean 4' 16"

M. MECHAIN's Observatory was 9' 23", and M. MESSIER's 9' 18" east of Greenwich.

Thus

Thus I have given a comparative view of the different methods I employed in settling the longitude of our Observatory, which is in Bootham, about 400 or 500 yards N. W. of the Minster. The occultations and meridian transits of the moon's limb, which make it $4' 25''\frac{1}{2}$, or $1^{\circ} 6' 25''$, would have been quite sufficient; but still it is interesting and useful to know how far the others err. With respect to the eclipses of the moon's spots, I think that method is in general too much neglected; and that it might be relied on infinitely more, if certain circumstances were mutually attended to.

1st, To be particular in specifying the clearness of the sky; for in hazy weather the results are very erroneous.

2dly, To chuse such spots that are well defined, and leave no hesitation as to the part eclipsed.

3dly, That every observer should, as much as possible, use telescopes equally powerful; at least let the magnifying powers be the same.

A principal objection may still be urged, *viz.* the difficulty of distinguishing the true shadow from the penumbra. Was this obviated, I believe, the results would be more exact than from Jupiter's first satellite: undoubtedly the shadow appears better defined if magnified little; but I am much inclined to think, that with high magnifying powers there is greater certainty of chusing the same part of the shadow, which perhaps is more than a sufficient compensation for the loss of distinctness.

Concerning the meridian observations of the moon's limb.

The advantages and precision of this method for determining the difference of meridians is, as I have already said, so little suspected,

suspected, that I flatter myself, the particulars I am going to mention will not be thought superfluous.

The rule I adopted is this:

The increase of the moon's R.A. in 12 hours (or any given time) found by computation, is to 12 hours as the increase of the moon's R.A. between two places, found by observation, is to the difference of meridians.

E X A M P L E.

November 30, 1782.

h.				
13 12	57,62	meridian transit of the moon's second limb	}	at Greenwich by clock.
13 13	29,08	ditto of a π		

31,46 Difference of R.A.

13 14	8,05	meridian transit of the moon's second limb.	}	at York by clock.
13 14	30,13	ditto of a π		

22,08	difference at York,	}	the clocks going nearly sidereal time no correction is required.
31,46	difference at Greenwich,		

9,38 increase of the moon's apparent R.A. between Greenwich and York, by observation.

141" in seconds of a degree, ditto, ditto, ditto,

The increase of the moon's R.A. for 12 hours by computation is 23340 seconds,
and 12 hours reduced into seconds is 43200;

therefore, according to the rule stated above,

$$23340'' : 43200'' :: 141'' : \text{difference of meridians} = 261''$$

These easy observations and short reduction are the whole of the business. Instead of computing the moon's R.A. for 12 hours, I have constantly taken it from the Nautical Almanacs, which give it sufficiently exact, provided some attention be paid to the increase or decrease of the moon's motion.

Were the following circumstances attended to, the results would undoubtedly be much more exact.

1st, Compare the observations to the same made in several other places.

2dly, Let several and the same stars be observed at these places.

3dly, Such stars as are nearest in R.A. and declination to the moon are infinitely preferable.

4thly, Your advice to get as near as possible an equal number of observations of each limb, to take a mean of each set, and then a mean of both means, cannot be too strongly urged. I am perfectly of your opinion, that it will considerably correct the error of telescopes and sight.

5thly, The adjustment of the telescopes to the eye of the observer before the observation, which you also recommend, will appear very judicious to every astronomer, who must have frequently perceived what you mention, that the sight is subject to vary.

6thly, As a principal error proceeds from the observation of the moon's limb, I think it may be considerably lessened, if certain little round spots near each limb were also observed in settled Observatories; in which case the libration of the moon will perhaps be a consideration.

7thly, When the difference of meridians, or of the latitudes of the places, is very considerable, the change of the moon's diameter becomes an equation.

Though such are the requisites to use this method with advantage, only one or two of them have been employed in the observations that I have reduced. Two thirds of these observations had not even the same stars observed at Greenwich and York; and yet none of the results, except a doubtful one, differ

differ 15'' from the mean ; therefore, I think, we may expect a still greater exactness, perhaps within 10'', if the above particulars be attended to.

When the same stars are not observed, it is necessary for the observers at both places to compute their R.A. from tables, in order to get the apparent R.A. of the moon's limb ; though this is not so satisfactory as by actual observation, still the difference will be trifling, provided the stars R.A.'s are accurately settled. Your catalogue undoubtedly may be depended on the most, and those stars preferred which have their proper motions ascertained. A few years ago, I had the pleasure of communicating to you the proper motion of β Virginis, which I found to be 1'',02 *per* year, increasing in R.A. * : was this unknown, and that star observed alone with the moon, it would occasion, at this time, a very considerable error.

I am also of opinion, that the same method can be put in practice by travellers with little trouble, and a transit instrument constructed so as to fix up with facility in any place. Though I have not considered this sufficiently, I shall, nevertheless, subjoin a few remarks that may engage others to turn their thoughts more fully to the subject.

It is not necessary, perhaps, that the instrument should be perfectly in the meridian to a few seconds of time, provided stars, nearly in the *same parallel of declination* with the moon, are observed : nay, I am inclined to think, that if the instrument deviates even a quarter or half of a degree, or more, sufficient exactness can be obtained, as a table might be com-

* Some time previous to this communication, I had found, by the comparison of my transit observations of α Aquilæ and β Virginis, that the latter had moved forward with a proper motion of 0'',91 of time, or of 13'',65 of R.A. from 1767 to 1783, in 16 years, or at the rate of 0'',853 a year, on supposition that the proper motion of α Aquilæ is 0'',57 a year forward.

puted, shewing the moon's parallax and motion for such deviation, which deviation may easily be found by the well known method of observing stars whose difference of declination is considerable.

As travellers very seldom meet with situations to observe stars near the pole, or find a proper object for determining the error of the line of collimation, I shall recommend the following idea, which, I believe, has never yet been noticed, and hope it will answer the purpose. Having computed the apparent R.A. of four, six, or more stars, which have nearly the same parallel of declination, observe half of them with the instrument inverted, and the other half when in its right position; if the difference of R.A.'s between each set by observation agree with the computation, there is no error; but if they disagree, half that disagreement is the error of the line of collimation. The same observations may also serve to determine whether the distance of the corresponding wires are equal. In case of necessity, each limb of the sun might be observed in the same manner, though probably with less precision. By a single trial I made above two years ago, the result was much more exact than I expected. *MAYER's Catalogue of Stars* will prove of great use to those that adopt the above method.

In such a number of observations, it is not surprising that a few should be erroneous; I have rejected only three.

A meridian transit of the moon's limb, August 18, 1782; δ Sagittarii was the only star observed at York; it gives for difference of meridians, 3 55

Perhaps the star has a proper motion, or a mistake of one second might have been made in marking the clock.

An immersion of Jupiter's first satellite, June 22, 1783, which make the difference of meridians, 3 42

The air was hazy both at Greenwich and at York.

Lastly, an occultation of a star of the ninth magnitude, immersed behind the dark limb of the moon, during the eclipse of Sept 10, 1783, at 11 h. 29' 6'' apparent time. M. MESSIER also observed it at 11 h. 50' 49'' $\frac{1}{2}$ apparent time at Paris: he determined its R.A. $349^{\circ} 22' 17''$, and declination $5^{\circ} 38' 23''$ south. M. GOODRICKE, who computes very accurately, finds it gives for difference of meridians, 4 44 $\frac{1}{2}$

I am rather surpris'd, that the immersions of known stars of the sixth and seventh magnitude behind the *dark limb* of the moon are not constantly observed in fixed Observatories, as they would frequently be of great use.

Latitude of York.

The following determinations for the latitude of York were made with a BIRD'S 18-inch quadrant, the telescope of two feet focus, with which instrument observations of the same star seldom differ 10''.

Latitude of the Observatory.

53	57	37	by 7 observations of Arcturus.
53	57	41	by 2 ditto of α Lyræ.
53	57	52	by 1 ditto of β Arietis.
53	57	37	by 1 ditto of β Cygni.
53	57	33	by 2 ditto of Algol.
53	57	57	by 4 ditto of γ Lyræ.
53	57	49	by 8 ditto of β Draconis.
53	57	46	by 6 ditto of μ Draconis.
53	57	56	by 2 ditto of γ Draconis,

53 57 45 + latitude on a mean.

The line of collimation was deduced from β , γ , and μ Draconis; half of each set observed with the face of the quadrant to the east, and half with its face to the west. This, as well as the other methods, is very tedious, particularly when required to be often repeated, as is the case in travelling; I shall therefore propose the following invention, the idea of which was improved on by Mr. SMEATON, and flatter myself it will prove of the greatest facility.

The error of the line of collimation includes the fixed errors of the instrument, and those that are subject to change, occasioned by the wires and glasses, &c. of the telescope moving. The error of these last may be found by making the telescope turn on its center, so that the sun, stars, or terrestrial objects may be observed on the horizontal wire in two manners; first, when the wire is in its natural position, and then inverted, which is performed by turning the telescope 180 degrees, or half round: thus, this part of the error can always be known with the greatest ease; and in order to find the fixed errors, it is requisite for *a single time* to get the *whole error* of the line of collimation by one of the common methods, from which the error of the telescope being deducted, the fixed errors become known; and as they are unchangeable, if any alteration should take place, it proceeds from the telescope, and may easily be detected as shewn above. Perhaps, instead of the whole telescope, it would be sufficient only to make that part turn containing the eye-glass and wires.

As the following observations made also at York may be of use, I beg, Sir, you will annex them to my paper on the longitude and latitude of that city, which lately I had the pleasure of sending you.

Dates.	App. time.	
	h. ' "	
1781, July 19	9 41 59	Emerfion of Jupiter's fecond fatellite; night fine.
1782, May 24	12 23 12	Immerfion of Jupiter's fecond fatellite; good.
July 20	11 27 40	Emerfion of Jupiter's 2d fat.; doubtful; air very hazy.
Nov. 30	20 57 16	Immerfion of α γ behind the moon; instantaneous.
	20 57 21 $\frac{1}{2}$ *	Ditto ditto; in another part of the town.
		Eclifp of the moon.
1783, Mar. 18	8 27 50	Total immerfion of the moon; air very clear.
	8 27 33*	Ditto; good.
	10 9 36	Moon begins to emerge; } air hazy.
	10 10 18	Certainly emerged; }
June 26	13 35 21	Immerfion of Jupiter's fecond fatellite; good.
	13 34 52*	Ditto; middling.
		Eclifp of the moon; air clear.
Sept. 10	9 30 45	Appearance of penumbra.
	12 17 30	Moon not emerged, but light ftrong.
	12 19 35	Ditto; very ftrong.
	12 21 14	Moon begins to emerge, but uncertain.
	12 21 44	Ditto; more certain.
	12 21 56*	Ditto; ditto.
	12 22 24	Moon certainly emerged.
	12 22 24*	Ditto.
	13 21 00	End of the eclifp, doubtful; air hazy.
	13 21 23*	Ditto.
	13 22 18*	Certainly ended, but not clear of penumbra.
	13 22 45	Ditto, ditto; air clearer.
		Several fots were obferved, but are here omitted, for fear of being too voluminous.
Sept. 16	10 22 41	{ Emerfion of Jupiter's fecond fatellite; air clear; but Jupiter low.
23	9 27 18	Emerfion of Jupiter's 3d fat.; Jupiter low; undulation.
Oct. 11	7 34 9*	Emerfion of Jupiter's fecond fatellite.
	7 34 21	Ditto; tolerably good.
29	5 42 53	Emerfion of Jupiter's third fatellite.
	5 46 16	Equal in brightnefs to the fecond fatellite; air clear.
1784, July 27	10 7 46	{ Immerfion of Jupiter's third fatellite; tolerably good, though undulation.

Dates.

Dates.	App. time.	
	h.	
1784, Aug. 26	8 54 12	Immersion of τ β behind the moon; instantaneous.
Oct. 11	9 49 30	{ Emerfion of Jupiter's fecond fatellite; good, though flight haze.
	9 49 26*	Ditto.
Nov. 12	9 33 59	Emerfion of Jupiter's fecond fatellite.
	9 34 1*	Ditto.
1785, July 15	12 26 50	Immersion of Jupiter's fecond fatellite; air clear.
Aug. 18	11 44 37	Immersion of Jupiter's third fatellite; good; { the air a
Sept. 17	12 16 55	Immersion of Jupiter's fecond fatellite; good; { little va-
Oct. 29	6 33 26	Emerfion of Jupiter's third fatellite; { pourifh.
Nov. 15	9 24 \pm	{ I examined Jupiter's fourth fatellite during 20', with- out being certain whether it had diminifhed in light.
Dec. 15	5 50 48	Immersion of 125 8 by the moon, exact within 3".

I have again marked with an afterifk the obfervations made by Mr. GOODRICKE, who defired me to communicate them. This worthy young man exifts no more; he is not only regretted by many friends, but will prove a lofs to aftronomy, as the difcoveries he fo rapidly made fufficiently evince: alfo his quicknefs in the ftudy of mathematics was well known to feveral perfons eminent in that line.

Declination of the needle.

	h		
1780, Sept. 13.	at $2\frac{1}{2}$, by a mean of 22 trials,	23 40 $\frac{1}{2}$	} Declination weft.
1782, Dec. 26.	at $0\frac{3}{4}$, by a mean of 16 trials,	23 5+	
1783, Nov. 14.	at $0\frac{1}{4}$, by a mean of 19 trials,	23 59-	
1784, Jan. 17.	at $0\frac{2}{3}$, by a mean of 13 trials,	23 54+	

Thefe obfervations were taken with all poffible exactnefs; the needle was four inches long, and made by DOLLOND.

Sir H. ENGLEFIELD, when at Scarborough, in Auguft and September, 1781, was fo kind as to obferve, at noon, the height of his barometer and thermometer. I alfo made fimilar obfervations

observations in the Observatory at York; from which, by eight comparisons, none disagreeing above 0,018 of an inch from the mean, I find, that the quicksilver at the sea stood 0,063 of an inch higher than at York. The barometers were made by RAMSDEN, and they agreed together to 0,005 part of an inch. We may later also expect to get the mean height of the barometer and thermometer, as there are several gentlemen that observe them every day, particularly Mr. WYVIL and Dr. WHITE at York, and Mr. CHOMONLEY at Bransby.

I remain, Sir, with great regard, &c.

EDW. PIGOTT.

May 26, 1786.

XXIV. *Advertisement of the expected Return of the Comet of 1532 and 1661 in the Year 1788. By the Rev. Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read June 29, 1786.

THE comet of 1531, 1607, and 1682, having returned in the year 1759, according to Dr. HALLEY's prediction in his *Synopsis Astronomiæ Cometicæ*, first published in the Philosophical Transactions in 1705, and re-published with his Astronomical Tables in 1749, there is no reason to doubt that all the other comets will return after their proper periods, according to the remark of the same author.

In the first edition of the *Synopsis* he supposed the comets of 1532 and 1661, from the similarity of the elements of their orbits, to be one and the same; but in the second edition he has seemed to lessen the weight of his first conjecture by not repeating it. Probably he thought it best to establish this new point in astronomy, the doctrine of the revolution of comets in elliptic orbits, as all philosophical matters in the beginning should be, on the most certain grounds; and feared that the vague observations of the comet, made by APIAN in 1532, might rather detract from, than add to, the evidence arising from more certain *data*. Astronomers, however, have generally acquiesced in his first conjecture of the comets of 1532 and 1661 being one and the same, and to expect its return to its perihelium accordingly in 1789.

The

The interval between the passages of the comet by the perihelium in 1532 and 1661 is 128 years, 89 days, 1 hour, 29 minutes (32 of the years being biffextile), which added to the time of the perihelium in 1661, together with 11 days to reduce it from the Julian to the Gregorian stile, which we now use, brings out the expected time of the next perihelium to be April 27th, 1 h. 10' in the year 1789.

The periodic times of the comet, which appeared in 1531, 1607, and 1682, having been of 76 and 75 years alternately, Dr. HALLEY supposed, that the subsequent period would be of 76 years; and that it would return in the year 1758; but, upon considering its near approach to Jupiter, in its descent towards the sun in the summer of 1681, he found, that the action of Jupiter upon the comet was, for several months together, equal to one-fiftieth part of the sun upon it, tending to increase the inclination of the orbit to the plane of the ecliptic, and lengthen the periodic time. Accordingly, the inclination of the orbit was found by the observations made in the following year 1682 to be 22' greater than in the year 1607. The effect of the augmentation of the periodic time could not be seen till the next return, which he supposed would be protracted by Jupiter's action to the latter end of the year 1758, or the beginning of 1759. M. CLAIRAUT, previous to its return, took the pains to calculate the actions both of Jupiter and Saturn on it during the whole periods from 1607 to 1682, and from 1682 to 1759, and thence predicted its return to its perihelium by the middle of April; it came about the middle of March, only a month sooner, which was a sufficient approximation to the truth in so delicate a matter, and did honour to this great mathematician, and his laborious calculations.

The comet in question is also, from the position of its orbit, liable to be much disturbed both by Jupiter and Saturn, particularly in its ascent from the sun after passing its perihelium, if they should happen to be near it, when it approaches to or crosses their orbits; because it is very near the plane of them at that time. When it passed the orbit of Jupiter in the beginning of February 1682, O. S. it was 50° *in consequentia* of that planet; and when it passed the orbit of Saturn in the beginning of October 1663, it was 17° *in consequentia* of it. Hence its motion would be accelerated while it was approaching towards the orbit of either planet by its separate action, and retarded when it had passed its orbit; but, as it would be subjected to the effect of retardation through a greater part of its orbit than to that of acceleration, the former would exceed the latter, and consequently the periodic time would be shortened; but probably not much, on account of the considerable distance of the comet from the planets when it passed by them; and therefore we may still expect it to return to its perihelium in the beginning of the year 1789, or the latter end of the year 1788, and certainly some time before the 27th of April 1789. But of this we shall be better informed after the end of this year, from the answers to the prize question proposed by the Royal Academy of Sciences at Paris, to compute the disturbances of the comet of 1532 and 1661, and thence to predict its return *.

* Since this was written, I received the unwelcome news, in a letter from M. MECHAIN, of the Royal Academy of Sciences at Paris, that the Academy has not received satisfactory answers concerning the disturbances of the comet between 1532 and 1661, and 1661 and the approaching return, and that the prize is referred to be adjudged of at Easter 1788, and that it will be 6000 livres. N. M.

If it should come to its perihelium on the 1st of January 1789, it might probably be visible, with a good achromatic telescope, in its descent to the sun, the middle of September 1788, and sooner or later, according as its perihelium should be sooner or later. It will approach us from the southern parts of its orbit, and therefore will first appear with considerable south latitude and south declination; so that persons residing nearer the equator than we do, or in south latitude, will have an opportunity of discovering it before us. It is to be wished that it may be first seen by some astronomer in such a situation, and furnished with proper instruments for settling its place in the heavens, the earliest good observations being most valuable for determining its elliptic orbit, and proving its identity with the comets of 1532 and 1661. The Cape of Good Hope would be an excellent situation for this purpose.

In order to assist astronomers in looking out for this comet, I have here given its heliocentric and geocentric longitudes and latitudes and correspondent distances from the sun and earth, on supposition that it shall come to its perihelium on January 1, 1789. But if that should happen sooner or later, the heliocentric longitudes and latitudes and distances from the sun will stand good if applied to days as much earlier or later, as the time of the perihelium may happen sooner or later; and the geocentric longitudes and latitudes and distances from the earth must be re-computed accordingly. The calculations are made for a parabolic orbit from the elements determined by Dr. HALLEY from HEVELIUS's observations in 1661, only allowing for the precession of the equinoxes. The elements made use of were as follows:

Time of perihelium January 1, 1789, at noon.

Perihelium distance 0,44851.

K k k 2

Place

Place of ascending node $2^{\circ} 24' 18''$.

Inclination of orbit to the ecliptic $32^{\circ} 36'$.

Perihelium forwarder in orbit than the ascending node $33^{\circ} 28'$.

Its motion is direct.

Computed places of the comet, on supposition that it shall return to its perihelium January 1, 1789, at noon.

Times.	Dist. from \odot .	Dist. from the earth.	Heliocentric longitude.	Heliocentric latitude.	Geocentric longitude.	Geocentric latitude.	Product of distances from \odot and earth.
1788			S. D. M.	D. M.	S. D. M.	D. M.	
Apr. 23, 7	4, 0	4,52	11 3 54	30 56 S	11 16 30	27 5 S	18,07
June 4, 1	3, 5	3,54	11 7 6	31 25	11 26 31	31 4	12,38
July 14, 5	3,	2,57	11 11 16	31 55	0 3 21	38 11	7,70
Aug. 2, 46	2,75	2,15	11 13 47	32 10	0 4 8	42 59	5,90
— 20, 43	2, 5	1,79	11 16 39	32 22	0 2 6	48 16	4,48
Sept. 7, 2	2,25	1,51	11 20 9	32 32	11 25 6	53 28	3,39
— 24, 6	2,	1,29	11 24 16	32 36	11 13 12	56 45	2,58
Oct. 10, 26	1,75	1,13	11 29 24	32 30	10 28 22	56 36	1,75
— 26, 64	1,50	1,01	0 5 51	32 4	10 15 50	52 6	1,51
Nov. 9, 34	1,25	0,88	0 14 19	31 0	10 8 36	46 47	1,10
— 23, 36	1, 0	0,76	0 26 4	28 32	10 4 10	39 0	0,76
Dec. 7, 21	0,75	0,62	1 13 58	22 29	9 29 18	27 45	0,46
— 23, 37	0,50	0,50	2 20 58	2 8	9 14 31	2 7 S	0,25
— 24, 35	0,49	0,51	2 24 18	0 0	9 12 58	0 0	0,25
1789							
Jan. 1, 6	0,45	0,59	3 23 25	17 17 N	9 2 50	13 8 N	0,26

The last observation made by HEVELIUS on the comet in 1661 was when its distance from the earth was 0,986, and from the sun 1,37, with what he calls a very long and good telescope; at which time it appeared faint and small with it, though

though still sufficiently visible. Let us suppose this to have been a telescope of 9-feet focal length, with an aperture of 1,65 inch; then, because the diameter of the aperture of a telescope sufficient to render the comet equally visible should be as the product of its distances from the sun and earth, and the product of the numbers above-mentioned 0,986 and 1,37 is 1,35, we shall have the following analogy to find the aperture of a refracting telescope sufficient to shew the comet as it appeared to HEVELIUS. As 1,35 : 1,65 inch :: 9 : 11 inches, so is the product of distances from the sun and earth to the diameter of the aperture required in inches.

XXV. *A new Method of finding Fluents by Continuation.* By
the Rev. Samuel Vince, A. M. F. R. S.

Read July 6, 1786.

ART. I. Put $\dot{F} = \frac{x^m \dot{x}}{a^n + x^n} = x^{m-n} \dot{x} - a^n x^{m-2n} \dot{x} + a^{2n} x^{m-3n} \dot{x} -$
 $\&c. \pm \frac{a^{rn} x^{m-rn} \dot{x}}{a^n + x^n}$, then $F = \frac{x^{m-n+1}}{m-n+1} - \frac{a^n x^{m-2n+1}}{m-2n+1} + \frac{a^{2n} x^{m-3n+1}}{m-3n+1} - \&c.$
 $\pm W$, where W represents the fluent of the last term. Now
 $\frac{x^m \dot{x}}{a^n + x^n} = \frac{x^m \dot{x} \times \overline{a^n + x^n}^{1-r}}{a^n + x^n}$; hence $\int \frac{x^m \dot{x}}{a^n + x^n} = \int \frac{x^m \dot{x} \times \overline{a^n + x^n}^{1-r}}{a^n + x^n} = F$
 $\times \overline{a^n + x^n}^{1-r} - \int F \times \frac{\overline{1-r} \cdot n x^{n-1} \dot{x}}{a^n + x^n} =$ (by substituting for F its
value in the latter quantity) $F \times \overline{a^n + x^n}^{1-r} - \frac{\overline{1-r} \cdot n}{m-n+1} \times$
 $\int \frac{x^m \dot{x}}{a^n + x^n} + \frac{\overline{1-r} \cdot n a^n}{m-2n+1} \times \int \frac{x^{m-n} \dot{x}}{a^n + x^n} - \frac{\overline{1-r} \cdot n a^{2n}}{m-3n+1} \times \int \frac{x^{m-2n} \dot{x}}{a^n + x^n} + \&c. \pm$
 $\int W \times \frac{\overline{1-r} \cdot n x^{m-1} \dot{x}}{a^n + x^n}$; transpose $-\frac{\overline{1-r} \cdot n}{m-n+1} \times \int \frac{x^m \dot{x}}{a^n + x^n}$ and divide
both sides of the equation by $\frac{m-rn+1}{m-n+1}$ and we have
 $\int \frac{x^m \dot{x}}{a^n + x^n} = \frac{m-n+1}{m-rn+1} \times F \times \overline{a^n + x^n}^{1-r} + \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r} \cdot n a^n}{m-2n+1} \times$
 $\int \frac{x^{m-n} \dot{x}}{a^n + x^n} - \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r} \cdot n a^{2n}}{m-3n+1} \times \int \frac{x^{m-2n} \dot{x}}{a^n + x^n} + \&c. \pm \frac{m-n+1}{m-rn+1} \times$

∫

$\int W \times \frac{1-r \cdot na^{n-1} \dot{x}}{a^n + x^n} \dot{x}$; now the fluent of the last term is

$$= \frac{m-n+1}{m-rn+1} \times W \times \overline{a^n + x^n}^{1-r} = \frac{m-n+1}{m-rn+1} \times \int \frac{a^{2n} x^{m-2n} \dot{x}}{a^n + x^n} \dot{x}; \text{ hence by}$$

substituting this quantity for the last term, it is manifest, that the first part $= \frac{m-n+1}{m-rn+1} \times W \times \overline{a^n + x^n}^{1-r}$ will be destroyed

by the last term of $\frac{m-n+1}{m-rn+1} \times F \times \overline{a^n + x^n}^{1-r}$, when we substitute

for F its value; hence if we put $M = \frac{x^{m-n+1}}{m-n+1} - \frac{a^n x^{m-2n-1}}{m-2n+1} + \frac{a^{2n} x^{m-3n+1}}{m-3n+1} - \&c.$ omitting the last term $\pm W$, we have

$$\int \frac{x^m \dot{x}}{a^n + x^n} = \frac{m-n+1}{m-rn+1} \times M \times \overline{a^n + x^n}^{1-r} + \frac{m-n+1}{m-rn+1} \times \frac{1-r \cdot na^n}{m-2n+1} \times$$

$$\int \frac{x^{m-n} \dot{x}}{a^n + x^n} - \frac{m-n+1}{m-rn+1} \times \frac{1-r \cdot na^{2n}}{m-3n+1} \times \int \frac{x^{m-2n} \dot{x}}{a^n + x^n} + \&c. = \frac{m-n+1}{m-rn+1} \times$$

$$\times a^{2n} \times \int \frac{x^{m-2n} \dot{x}}{a^n + x^n}; \text{ hence, if the fluent of the last term be}$$

given, we have the general law of continuation by which we

may find the fluent of $\frac{x^m \dot{x}}{a^n + x^n}$. If the fluxion be $\frac{x^m \dot{x}}{a^n + x^n}$ all the

terms after the first will be negative, and the last always positive.

Ex. 1. Given the fluent of $\frac{\dot{x}}{\sqrt{1+x^2}}$ to find the fluent of

$$\frac{x^{2r} \dot{x}}{\sqrt{1+x^2}}.$$

Here $n=2$, $a=1$, $m=2r$, $r=\frac{1}{2}$, $M = \frac{x^{2r-1}}{2r-1} - \frac{x^{2r-3}}{2r-3} + \&c.$ to

$\pm x$,

$\pm x$, and the fluent of $\frac{x^s}{\sqrt{1+x^2}}$ is the hyp. log. $x + \sqrt{1+x^2}$

which call Q; hence $\int \frac{x^{2s}}{\sqrt{1+x^2}} = \frac{2s-1}{2s} \times M \times \sqrt{1+x^2} + \frac{2s-1}{2s \cdot 2s-3}$

$$\times \int \frac{x^{2s-2}}{\sqrt{1+x^2}} - \frac{2s-1}{2s \cdot 2s-5} \times \int \frac{x^{2s-4}}{\sqrt{1+x^2}} + \&c. \pm \frac{2s-1}{2s} \times Q.$$

$$\text{If } s=1, \int \frac{x^2}{\sqrt{1+x^2}} = \frac{1}{2} \sqrt{1+x^2} \times x - \frac{1}{2} Q = \alpha.$$

$$s=2, \int \frac{x^4}{\sqrt{1+x^2}} = \frac{1}{4} \sqrt{1+x^2} \times \frac{x^3}{3} - x + \frac{1}{4} \alpha + \frac{1}{4} Q = \beta.$$

$$s=3, \int \frac{x^6}{\sqrt{1+x^2}} = \frac{5}{6} \sqrt{1+x^2} \times \frac{x^5}{5} - \frac{x^3}{3} + x + \frac{5}{6} \beta - \frac{5}{6} \alpha - \frac{5}{6} Q = \gamma$$

$$s=4, \int \frac{x^8}{\sqrt{1+x^2}} = \frac{7}{8} \sqrt{1+x^2} \times \frac{x^7}{7} - \frac{x^5}{5} + \frac{x^3}{3} - x + \frac{7}{8} \gamma - \frac{7}{8} \beta$$

$$+ \frac{7}{8} \alpha + \frac{7}{8} Q.$$

&c.

&c.

&c.

Ex. 2. To find the fluent of $x^{\frac{s}{2}} \dot{x} \sqrt{2+x}$, given the fluent of $x^{-\frac{1}{2}} \dot{x} \sqrt{2+x}$, and s an odd number.

Here $a=2$, $n=1$, $r=-\frac{1}{2}$, $\frac{s}{2}=m$, $m-vn=-\frac{1}{2}$, or $\frac{s}{2}-v$

$$=-\frac{1}{2}; \therefore v=\frac{s+1}{2}, M=\frac{2x^{\frac{s}{2}}}{s} - \frac{4x^{\frac{s}{2}-1}}{s-2} + \frac{8x^{\frac{s}{2}-2}}{s-4} - \&c. \text{ and the}$$

fluent (Q) of $x^{-\frac{1}{2}} \dot{x} \sqrt{2+x}$ is $\pi + \sqrt{2x+x^2}$, where π = hyp.

log. $1+x+\sqrt{2x+x^2}$; hence $\int x^{\frac{s}{2}} \dot{x} \sqrt{2+x} = \frac{s}{s+3} \times \sqrt{2+x}^{\frac{s}{2}} \times$

$$M + \frac{s}{s+3} \times \frac{b}{s-2} \times \int x^{\frac{s}{2}-1} \dot{x} \sqrt{2+x} - \frac{s}{s+3} \times \frac{12}{s-4} \times \int x^{\frac{s}{2}-2} \dot{x} \sqrt{2+x}$$

$$+ \frac{s}{s+3} \times \frac{24}{s-6} \times \int x^{\frac{s}{2}-3} \dot{x} \sqrt{2+x} \quad \&c. \pm \frac{s}{s+3} \times 2^{\frac{s+1}{2}} \times \int x^{-\frac{1}{2}} \dot{x}$$

$$\sqrt{2+x}.$$

If

$$\text{If } s=1, \int x^{\frac{1}{2}} \sqrt{2+x} = \frac{1}{2} \times \overline{2+x}^{\frac{3}{2}} \times 2x^{\frac{1}{2}} - \frac{1}{2} Q = \alpha.$$

$$s=3, \int x^{\frac{3}{2}} \sqrt{2+x} = \frac{1}{2} \times \overline{2+x}^{\frac{3}{2}} \times \frac{2x^{\frac{3}{2}}}{3} - 4x^{\frac{1}{2}} + 3\alpha + 2Q = \beta.$$

$$s=5, \int x^{\frac{5}{2}} \sqrt{2+x} = \frac{1}{2} \times \overline{2+x}^{\frac{3}{2}} \times \frac{2x^{\frac{5}{2}}}{5} - \frac{4x^{\frac{3}{2}}}{3} + 8x^{\frac{1}{2}} + \frac{5}{4} \beta -$$

$$\frac{15}{2} \alpha - 5Q = \gamma.$$

$$s=7, \int x^{\frac{7}{2}} \sqrt{2+x} = \frac{1}{2} \times \overline{2+x}^{\frac{3}{2}} \times \frac{2x^{\frac{7}{2}}}{7} - \frac{4x^{\frac{5}{2}}}{5} + \frac{8x^{\frac{3}{2}}}{3} - 16x^{\frac{1}{2}} +$$

$$\frac{21}{25} \gamma - \frac{14}{5} \beta + \frac{84}{5} \alpha + \frac{56}{5} Q.$$

&c.

&c.

&c.

$$\text{II. Let } \frac{x^n}{a+bx^m+x^{2m}} = x^{n-2m} - Px^{n-3m} + Qx^{n-4m} - \&c. \pm$$

$$\frac{Vx^{n-2m}}{a+bx^m+x^{2m}} \pm \frac{Wx^{n-3m}}{a+bx^m+x^{2m}}, \text{ then } F = \frac{x^{n-2m+1}}{n-2m+1} - \frac{Px^{n-3m+1}}{n-3m+1} + \frac{Qx^{n-4m+1}}{n-4m+1}$$

- &c. $\pm T \pm U$, where T and U are put for the fluents of the two last terms, and P, Q, &c. for the co-efficients arising from

the division. Now, $\int \frac{x^n}{\sqrt{a+bx^m+x^{2m}}} = \int \frac{x^n \times \overline{a+bx^m+x^{2m}}^{\frac{1}{2}}}{a+bx^m+x^{2m}} =$

$$F \times \overline{a+bx^m+x^{2m}}^{\frac{1}{2}} - \int F + \frac{1-r \cdot mbx^{m-1} + 1-r \cdot 2mx^{2m-1}}{a+bx^m+x^{2m}} = (\text{by}$$

substituting for F its value in the latter quantity, and putting A, B, C, &c for the co-efficients which arise in consequence

thereof) $F \times \overline{a+bx^m+x^{2m}}^{\frac{1}{2}} - A \times \int \frac{x^n}{\overline{a+bx^m+x^{2m}}^{\frac{1}{2}}} + B \times$

$$\int \frac{x^{n-m}}{\overline{a+bx^m+x^{2m}}^{\frac{1}{2}}} - C \times \int \frac{x^{n-2m}}{\overline{a+bx^m+x^{2m}}^{\frac{1}{2}}} + \&c.$$

$$= \int T \times \frac{\overline{1-r} \cdot mbx^{m-1} \dot{x} + \overline{1-r} \cdot 2mx^{2m-1} \dot{x}}{a + bx^m + x^{2m}} \dot{x}$$

$$= \int U \times \frac{\overline{1-r} \cdot mbx^{m-1} \dot{x} + \overline{1-r} \cdot 2mx^{2m-1} \dot{x}}{a + bx^m + x^{2m}} \dot{x}; \text{ hence by transposition}$$

and division we have $\int \frac{x^n \dot{x}}{a + bx^m + x^{2m}} = \frac{1}{1+A} \times F \times \overline{a + bx^m + x^{2m}}^{1-r}$

$$+ \frac{B}{1+A} \times \int \frac{x^{n-m} \dot{x}}{a + bx^m + x^{2m}} - \frac{C}{1+A} \times \int \frac{x^{n-2m} \dot{x}}{a + bx^m + x^{2m}} + \&c.$$

$$= \int \frac{T}{1+A} \times \frac{\overline{1-r} \cdot mbx^{m-1} \dot{x} + \overline{1-r} \cdot 2mx^{2m-1} \dot{x}}{a + bx^m + x^{2m}} \dot{x}$$

$$= \int \frac{U}{1+A} \times \frac{\overline{1-r} \cdot mbx^{m-1} \dot{x} + \overline{1-r} \cdot 2mx^{2m-1} \dot{x}}{a + bx^m + x^{2m}} \dot{x}. \text{ Now the fluents}$$

of these two last terms are $= \frac{T}{1+A} \times \overline{a + bx^m + x^{2m}}^{1-r} = \frac{V}{1+A}$

$$\int \frac{x^{n-2m} \dot{x}}{a + bx^m + x^{2m}} \text{ and } = \frac{U}{1+A} \times \overline{a + bx^m + x^{2m}}^{1-r} = \frac{W}{1+A} \int \frac{x^{n-1+1 \cdot m} \dot{x}}{a + bx^m + x^{2m}}$$

respectively; hence, by substituting these for the last term,

it is manifest that $= \frac{T}{1+A} \times \overline{a + bx^m + x^{2m}}^{1-r}$ and $= \frac{U}{1+A}$

$\times \overline{a + bx^m + x^{2m}}^{1-r}$ will be destroyed by the two last terms of

$\frac{1}{1+A} \times F \times \overline{a + bx^m + x^{2m}}^{1-r}$ when we substitute for F its value;

hence, if we put $M = \frac{x^{n-2m-1}}{n-2m+1} - \frac{Px^{n-3m+1}}{n-3m+1} + \frac{Qx^{n-4m+1}}{n-4m+1} - \&c.$

omitting the two last terms $\pm T$ and $\pm U$, we shall have

$$\int \frac{x^n \dot{x}}{a + bx^m + x^{2m}} = \frac{1}{1+A} \times M \times \overline{a + bx^m + x^{2m}}^{1-r} + \frac{B}{1+A} \times$$

$$\int \frac{x^{n-m} \dot{x}}{a + bx^m + x^{2m}} - \frac{C}{1+A} \times \int \frac{x^{n-2m} \dot{x}}{a + bx^m + x^{2m}} + \&c. = \frac{V}{1+A} \times$$

$\int \frac{x^{n-1} \dot{x}}{a+bx^m+x^{2m}} = \frac{W}{1+A} \times \frac{x^{n-1+1 \cdot m} \dot{x}}{a+bx^m+x^{2m}}$; hence if the two last fluents be given, we have the general law of continuation up to $\frac{x^n \dot{x}}{a+bx^m+x^{2m}}$ in the same manner as before.

III. In general, if we proceed as in the two last articles, we shall find $\int \frac{x^n \dot{x}}{a+bx^m+\&c. x^{tm}} = \frac{M}{P} \times \frac{x^{n-1+1 \cdot m} \dot{x}}{a+bx^m+\&c. x^{tm}} + \frac{A}{P}$

$\times \int \frac{x^{n-1} \dot{x}}{a+bx^m+\&c. x^{tm}} + \frac{B}{P} \times \int \frac{x^{n-2} \dot{x}}{a+bx^m+\&c. x^{tm}} + \&c. \pm \frac{T}{P}$
 $\times \int \frac{x^{n-1} \dot{x}}{a+bx^m+\&c. x^{tm}} \pm \frac{V}{P} \times \int \frac{x^{n-1+1 \cdot m} \dot{x}}{a+bx^m+\&c. x^{tm}} \pm \&c.$ where the

number of these last terms is t , and $M = \frac{x^{n-tm+1} \dot{x}}{n-tm+1} - \frac{Qx^{n-t+1 \cdot m} \dot{x}}{n-t+1 \cdot m+1}$ + &c. omitting, as before, the terms at the end arising from the remainders. Hence if the last t fluents be given, we can by continuation find the required fluent.

Because the division of $\frac{x^n \dot{x}}{a+bx^m+\&c. x^{tm}}$ may be expressed by an ascending series $x^n \dot{x} - Qx^{n+m} \dot{x} + Rx^{n+2m} \dot{x} - \&c.$ it is manifest, that by the same method we may continue the fluents downwards as well as upwards.

IV. Let $\dot{F} = \frac{x^n \dot{x}}{1-x} = -x^{n-1} \dot{x} - x^{n-2} \dot{x} - x^{n-3} \dot{x} - \&c. - x^{n-r} \dot{x} + \frac{x^{n-r} \dot{x}}{1-x}$, then $F = -\frac{x^n}{n} - \frac{x^{n-1}}{n-1} - \frac{x^{n-2}}{n-2} - \&c. - \frac{x^{n-r+1}}{n-r+1} + W$, where

W is the fluent of the last term. Now $\frac{x^n \dot{x}}{\sqrt{1-x^2}} = \frac{x^n \dot{x}}{1-x} \times \sqrt{\frac{1-x}{1+x}}$, hence

hence $\int \frac{x^n \dot{x}}{\sqrt{1-x^2}} = \int \frac{x^n \dot{x}}{1-x} \times \sqrt{\frac{1-x}{1+x}} = F \times \sqrt{\frac{1-x}{1+x}} + \int F \times$

$\frac{x^n \dot{x}}{\sqrt{1+x^2} \times 1+x} = F \times \sqrt{\frac{1-x}{1+x}} - \int \frac{x^n \dot{x}}{n \sqrt{1-x^2} \times 1+x} - \int \frac{x^{n-1} \dot{x}}{(n-1) \sqrt{1-x^2} \times 1+x}$
 $- \int \frac{x^{n-2} \dot{x}}{(n-2) \sqrt{1-x^2} \times 1+x} - \&c. - \int \frac{x^{n-r+1} \dot{x}}{(n-r+1) \sqrt{1-x^2} \times 1+x} +$

$\int W \times \frac{x^n \dot{x}}{\sqrt{1-x^2} \times 1+x}. \text{ But}$

$$\begin{aligned} \frac{x^n \dot{x}}{n \sqrt{1-x^2}} &= \frac{x^{n-1} \dot{x}}{n \sqrt{1-x^2}} + \frac{x^n \dot{x}}{n \sqrt{1-x^2}} - \&c. \mp \frac{x^{n-r} \dot{x}}{n \sqrt{1-x^2}} \pm \frac{x^{n-r} \dot{x}}{n \sqrt{1-x^2} \times 1+x} \\ \frac{x^{n-1} \dot{x}}{(n-1) \sqrt{1-x^2}} &= \frac{x^{n-2} \dot{x}}{(n-1) \sqrt{1-x^2}} + \frac{x^{n-1} \dot{x}}{(n-1) \sqrt{1-x^2}} + \&c. \pm \frac{x^{n-r} \dot{x}}{(n-1) \sqrt{1-x^2}} \mp \frac{x^{n-r} \dot{x}}{(n-1) \sqrt{1-x^2} \times 1+x} \\ \frac{x^{n-2} \dot{x}}{(n-2) \sqrt{1-x^2}} &= \frac{x^{n-3} \dot{x}}{(n-2) \sqrt{1-x^2}} - \&c. \mp \frac{x^{n-r} \dot{x}}{(n-2) \sqrt{1-x^2}} \pm \frac{x^{n-r} \dot{x}}{(n-2) \sqrt{1-x^2} \times 1+x} \\ \&c. & \qquad \qquad \qquad \&c. \\ \frac{x^{n-r+1} \dot{x}}{(n-r+1) \sqrt{1-x^2} \times 1+x} &= \frac{x^{n-r} \dot{x}}{(n-r+1) \sqrt{1-x^2}} - \frac{x^{n-r} \dot{x}}{(n-r+1) \sqrt{1-x^2} \times 1+x} \end{aligned}$$

Hence $\int \frac{x^n \dot{x}}{\sqrt{1-x^2}} = F \times \sqrt{\frac{1-x}{1+x}} - \frac{1}{n} \times \int \frac{x^{n-1} \dot{x}}{\sqrt{1-x^2}} + \frac{1}{n} - \frac{1}{n-1} \times$

$\int \frac{x^{n-2} \dot{x}}{\sqrt{1-x^2}} - \frac{1}{n} - \frac{1}{n-1} + \frac{1}{n-2} \times \int \frac{x^{n-3} \dot{x}}{\sqrt{1-x^2}} + \&c. \pm \frac{1}{n} \mp \frac{1}{n-1} \pm \&c.$

$- \frac{1}{n-r+1} \times \int \frac{x^{n-r} \dot{x}}{\sqrt{1-x^2}} \mp \frac{1}{n} \pm \frac{1}{n-1} \mp \&c. + \frac{1}{n-r+1} \times \int \frac{x^{n-r} \dot{x}}{\sqrt{1-x^2} \times 1+x}$

$+ \int W \times \frac{x^n \dot{x}}{\sqrt{1-x^2} \times 1+x}. \text{ Now } \int W \times \frac{x^n \dot{x}}{\sqrt{1-x^2} \times 1+x} = -W \times$

$\left[\frac{1-x}{1+x} \right]^{\frac{1}{2}} + \int \frac{x^{n-r} \dot{x}}{\sqrt{1-x^2}}; \text{ also } \int \frac{x^{n-r} \dot{x}}{\sqrt{1-x^2} \times 1+x} = -x^{n-r} \times \sqrt{\frac{1-x}{1+x}} +$

$\overline{n-r} \times \int \frac{x^{n-r-1} \dot{x}}{\sqrt{1-x^2}} - \overline{n-r} \times \int \frac{x^{n-r} \dot{x}}{\sqrt{1-x^2}}; \text{ hence, by substituting}$

these quantities in the two last terms, it is manifest that

$-W \times \left[\frac{1-x}{1+x} \right]^{\frac{1}{2}}$ will be destroyed by the last term of

F x

$F \times \sqrt{\frac{1-x}{1+x}}$ when we substitute for F its value; therefore, if we

put $M = -\frac{x^n}{n} - \frac{x^{n-1}}{n-1} - \frac{x^{n-2}}{n-2} - \&c. - \frac{x^{n-r+1}}{n-r+1}$, we shall have

$$\begin{aligned} \int \frac{x^n \dot{x}}{\sqrt{1+x^2}} &= M \pm \frac{1}{n} \mp \frac{1}{n-1} \pm \&c. - \frac{1}{n-r+1} \times \sqrt{\frac{1-x}{1+x}} \Big| - \frac{1}{n} \times \int \frac{x^{n-1} \dot{x}}{\sqrt{1-x^2}} \\ &+ \frac{1}{n-1} \times \int \frac{x^{n-2} \dot{x}}{\sqrt{1-x^2}} - \frac{1}{n-2} + \frac{1}{n-2} \times \int \frac{x^{n-3} \dot{x}}{\sqrt{1-x^2}} + \&c. \\ &\pm \frac{1}{n} \mp \frac{1}{n-1} \pm \&c. - \frac{1}{n-r+1} \times \overline{n-r+1+1} \times \int \frac{x^{n-r} \dot{x}}{\sqrt{1-x^2}} \mp \\ &\frac{1}{n} \pm \frac{1}{n-1} \mp \&c. + \frac{1}{n-r+1} \times \overline{n-r} \times \int \frac{x^{n-r-1} \dot{x}}{\sqrt{1-x^2}}. \end{aligned}$$

Hence, if the two last fluents be given, we have the general law of conti-

nuation up to the fluent of $\frac{x^n \dot{x}}{\sqrt{1-x^2}}$, where the index of x with-

out the vinculum increases by unity each time. And in the same manner we may (by increasing the index of x without by

m) find the fluent of $\frac{x^n \dot{x}}{\sqrt{a^{2m}-x^{2m}}}$ if we have given the fluents of

$$\frac{x^{n-2m} \dot{x}}{\sqrt{a^{2m}-x^{2m}}} \text{ and } \frac{x^{n-4m} \dot{x}}{\sqrt{a^{2m}-x^{2m}}}. \text{ Thus we have a general law of con-}$$

tinuation, where the index of x without is increased by half the index under the vinculum.

$$\text{V. Assume } F = \frac{x^n \dot{x}}{x^m - b} = x^{n-m} \dot{x} + b x^{n-2m} \dot{x} + b^2 x^{n-3m} \dot{x} + \&c. +$$

$$\frac{b^r x^{n-rm} \dot{x}}{x^m - b}, \text{ then } F = \frac{x^{n-m+1}}{n-m+1} + \frac{b x^{n-2m+1}}{n-2m+1} + \frac{b^2 x^{n-3m+1}}{n-3m+1} + \&c. + W,$$

where W is put for the fluent of the last term. Now

$$\int x^n \dot{x} \sqrt{\frac{x^m - a}{x^m - b}} = \int \frac{x^n}{x^m - b} \times \sqrt{x^m - a} \times \sqrt{x^m - b} = F \times \sqrt{x^m - a} \times \sqrt{x^m - b}$$

$$\begin{aligned}
 & - \int F \times \frac{2mx^{2m-1}\dot{x} - a + b \cdot mx^{m-1}\dot{x}}{2\sqrt{x^m - a \times x^m - b}} = (\text{by substituting for } F \text{ its} \\
 & \text{value in the latter quantity, and putting } A, B, C, \&c. \text{ for} \\
 & \text{the co-efficients which arise therefrom}) F \times \sqrt{x^m - a \times x^m - b} - \\
 & A \int \frac{x^n \dot{x}}{\sqrt{x^m - a \times x^m - b}} - B \int \frac{x^{n-m} \dot{x}}{\sqrt{x^m - a \times x^m - b}} - C \int \frac{x^{n-2m} \dot{x}}{\sqrt{x^m - a \times x^m - b}} - \\
 & \&c. - \int W \times \frac{2mx^{2m-1}\dot{x} - a + b \cdot mx^{m-1}\dot{x}}{2\sqrt{x^m - a \times x^m - b}}. \text{ Now } \frac{x^n \dot{x}}{\sqrt{x^m - a \times x^m - b}} = \\
 & \frac{x^n \dot{x}}{x^m - a} \sqrt{\frac{x^m - a}{x^m - b}}, \frac{x^{n-m} \dot{x}}{\sqrt{x^m - a \times x^m - b}} = \frac{x^{n-m} \dot{x}}{x^m - a} \sqrt{\frac{x^m - a}{x^m - b}}, \frac{x^{n-2m} \dot{x}}{\sqrt{x^m - a \times x^m - b}} = \frac{x^{n-2m} \dot{x}}{x^m - a} \\
 & \sqrt{\frac{x^m - a}{x^m - b}}, \&c. \text{ But}
 \end{aligned}$$

$$\begin{aligned}
 \frac{x^n \dot{x}}{x^m - a} &= x^{n-m} \dot{x} + ax^{n-2m} \dot{x} + a^2 x^{n-3m} \dot{x} + \&c. + a^{r-1} x^{n-rm} \dot{x} + \frac{a^r x^{n-rm} \dot{x}}{x^m - a} \\
 \frac{x^{n-m} \dot{x}}{x^m - a} &= x^{n-2m} \dot{x} + ax^{n-3m} \dot{x} + \&c. + a^{r-2} x^{n-rm} \dot{x} + \frac{a^{r-1} x^{n-rm} \dot{x}}{x^m - a} \\
 \frac{x^{n-2m} \dot{x}}{x^m - a} &= x^{n-3m} \dot{x} + \&c. + a^{r-3} x^{n-rm} \dot{x} + \frac{a^{r-2} x^{n-rm} \dot{x}}{x^m - a} \\
 \&c. & \qquad \qquad \&c. \qquad \qquad \&c.
 \end{aligned}$$

$$\begin{aligned}
 \text{Hence, by substitution, } \int x^n \dot{x} \sqrt{\frac{x^m - a}{x^m - b}} &= F \times \sqrt{x^m - a \times x^m - b} \\
 &- A \int x^{n-m} \dot{x} \sqrt{\frac{x^m - a}{x^m - b}} - \overline{Aa + B} \times \int x^{n-2m} \dot{x} \sqrt{\frac{x^m - a}{x^m - b}} - \overline{Aa^2 + Ba + C} \\
 &\times \int x^{n-3m} \dot{x} \sqrt{\frac{x^m - a}{x^m - b}} - \&c. - \overline{Aa^{r-1} + Ba^{r-2} + Ca^{r-3} + \&c.} \times \\
 &\times \int x^{n-rm} \dot{x} \sqrt{\frac{x^m - a}{x^m - b}} - \overline{Aa^r + Ba^{r-1} + Ca^{r-2} + \&c.} \times \int \frac{x^{n-rm} \dot{x}}{\sqrt{x^m - a \times x^m - b}} \\
 &- \int W \times \frac{2mx^{2m-1}\dot{x} - a + b \cdot mx^{m-1}\dot{x}}{2\sqrt{x^m - a \times x^m - b}}. \text{ But}
 \end{aligned}$$

$$= \int W \times \frac{2mx^{2m-1}\dot{x} - \frac{1}{2} + b \cdot mx^{m-1}\dot{x}}{2\sqrt{x^m-a} \times x^m-b} \text{ is } -W \times \sqrt{x^m-a} \times x^m-b$$

$$+ b \times \int x^{n-rm}\dot{x} \sqrt{\frac{x^m-a}{x^m-b}}; \text{ hence, by substituting this for the last}$$

$$\text{term, it is manifest, that } -W \times \sqrt{x^m-a} \times x^m-b \text{ will be de-}$$

$$\text{stroyed by the last term of } F \times \sqrt{x^m-a} \times x^m-b \text{ when we sub-}$$

$$\text{stitute for } F \text{ its value; therefore, if we put } M = \frac{x^{n-r+1}}{n-m+1} +$$

$$\frac{bx^{n-2m+1}}{n-2m+1} + \&c. + \frac{b^{r-1}x^{n-rm+1}}{n-rm+1}, \text{ we have } \int x^n\dot{x} \sqrt{\frac{x^m-a}{x^m-b}} = M \times$$

$$\sqrt{x^m-a} \times x^m-b - A \int x^{n-m}\dot{x} \sqrt{\frac{x^m-a}{x^m-b}} - Aa + B \times \int x^{n-2m}\dot{x}$$

$$\sqrt{\frac{x^m-a}{x^m-b}} - Aa^2 + Ba + C \times \int x^{n-3m}\dot{x} \sqrt{\frac{x^m-a}{x^m-b}} - \&c. -$$

$$Aa^{r-1} + Ba^{r-2} + \&c. - b^r \times \int x^{n-rm}\dot{x} \sqrt{\frac{x^m-a}{x^m-b}} - Aa^r + Ba^{r-1} + Ca^{r-2}$$

$$+ \&c. \times \int \frac{x^{n-rm}\dot{x}}{\sqrt{x^m-a} \times x^m-b}. \text{ Hence if the last two fluents be}$$

$$\text{given, we have the general law of continuation up to the}$$

$$\text{fluent of } x^n\dot{x} \sqrt{\frac{x^m-a}{x^m-b}}.$$

The utility of finding fluents by continuation was manifest to Sir ISAAC NEWTON, who first proposed it; and since his time some of the most eminent mathematicians have employed much of their attention upon it. The method which I have investigated and exemplified in this Paper I offer as being entirely new; and at the same time it not only exhibits, at once, the general law up to the required fluent, but also appears, from some of the instances here given, to be more extensive and convenient in its application than any method hitherto offered.

The

The general resolution of the given fluxion into a series of fluxions of the same kind, where the index of the unknown quantity without the vinculum keeps decreasing or increasing either by the index under or by half the index, has not, that I know of, before been given; which furnishes us at once not only with a very easy method of *continuing* fluents, but also points out a very simple method of investigating the fluent of the given fluxion *without* continuation. For if $\int \dot{A} = p + b \int \dot{B} + c \int \dot{C} + d \int \dot{D} + \&c.$ $\int \dot{B} = p' + c' \int \dot{C} + d' \int \dot{D} + \&c.$ $\int \dot{C} = p'' + d'' \int \dot{D} + \&c. \&c. \&c.$ then if for $\int \dot{B}$, $\int \dot{C}$, &c. &c. we substitute their respective values, we shall get a general series for $\int \dot{A}$ without continuation. The extent of any new method is, at first, seldom obvious; and how far that which is here proposed may be successfully employed in other cases will best appear from its application. Different methods will always be found to have their uses in particular cases; for where one becomes impracticable another will often be found to succeed; and I hope that which is here offered will contribute something towards facilitating the investigation of fluents.

XXVI. *Conjectures relative to the Petrifications found in St. Peter's Mountain, near Maestricht. By Petrus Camper, M. D. F. R. S.*

Read July 6, 1786.

THE discovery of a great number of petrified bones about the year 1770, in the mountain of St. Peter at Maestricht, and particularly of large jaw-bones with their teeth, suggested to the late M. HOFFMAN, first Surgeon to the Military Hospital at Maestricht, a worthy member of several learned Societies, and a great admirer of natural history, the idea that these maxillæ belonged to crocodiles. This notion was spread by himself and his literary correspondents through all Europe.

He did me the favour to send me, not only the history of those petrifications, but also several figures of the jaw-bones in question, and of other bones, which were all intirely new to me, except some fragments of the bones of turtles. I discovered, however, at the very first sight, the characteristical differences which distinguished these bones from those of crocodiles, of which I had at that time several in my collection.

His intention was to write upon this subject, and to send his essay, containing his reasons for supposing these bones to belong to crocodiles, to the Royal Society; but I dissuaded him, as a friend, from doing this, lest he should afterwards be under a necessity of retracting his opinion: and I sent him a figure of

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the lower jaw of a crocodile, accurately done by my own hand, and soon after the skull and under jaw of a pretty large crocodile; which induced him to defer his design of writing about these antiquities of the old world, until he should be better informed on the subject of cetaceous fishes.

Major DROVIN, of Maestricht, who made, about the same time, a collection of an infinite variety of corals, madrepores, alcyoniums, echinites, belemnites, shells, and petrified wood, from the same mountain and its environs, likewise procured a beautiful specimen of two maxillary bones of the same incognitum, but with the insides turned outwards; and this gentleman also supposed them to belong to the crocodile. A sketch of this specimen is to be found in M. BUCHOZ's *Dons de la Nature*, tab. 68. But the specimen itself is now in TEYLER's Museum, at Haerlem, with the whole of Major DROVIN's collection.

Another still more valuable and perfect specimen is to be seen at the house of the reverend Dean GODDING, of which there is likewise a rough sketch in M. BUCHOZ's *Dons de la Nature*, pl. 66. In this the greater part of both the upper and under maxillary bones is intire, and a bone, with small teeth, belonging to the palate; by which it appears, the animal had not only teeth in the jaw-bones, but also in the throat, as several fishes have, but which are never found in the mouth of crocodiles.

Notwithstanding all my endeavours to convince my friends, and afterwards M. DROVIN, and particularly the Dean, whose valuable and truly beautiful specimens I saw in the year 1782, I never could prevail upon them to adopt my opinion, that these bones belonged to physeteres or respiring fishes. M. HOFFMANN, adhering closely to the Linnæan System, ob-
jected,

jected, that the physeteres had teeth only in the lower jaw-bone, whereas this fossil monster had them in both upper and lower maxilla. He did not seem to recollect, that *φυσήτης* signifies something respiring, or breathing, and applied to fishes, *breathing fishes*; nor that the physeteres, according to the Linnæan system, have small teeth in the upper jaw-bone, though larger ones in the lower jaw, according to the observations of Dr. OTHO FABRICIUS, in his *Fauna Groenlandica*, p. 42. where he mentions the *macrocephalus*, and p. 45. where he speaks of the *microps*.

In August 1782, I sent M. GODDING, who had favoured me with a copy of his valuable specimen, a full demonstration of its being the head of a physeter, or breathing fish, Delphinus, or Orca, or under whatever genus it may be ranked, as having large teeth of the same size in both the maxillæ. But in vain; for he continues still to call it a crocodile, as if its value depended upon the species of the animal.

The analogy of all the other marine bodies seems to make it still more probable, that these large bones belong to the inhabitants of the sea, and not of rivers. The large turtles, the numberless echinites, madrepores, shells, alcyoniums, belemnites, orthoceratites, and so on, are all sea animals; and the crocodile would, in that case, be the only inhabitant of the rivers mixed with them.

The pretended crocodile found near Whitby, in Yorkshire, *Phil. Transf. vol. L. p. II. 1758, § 92. p. 688. and ibid. § 108. p. 786.* is undoubtedly the skeleton of a *Balæna*.

§ 2. After the decease of M. HOFFMAN, his family having offered the whole collection for sale, I went in August 1782 to Maestricht on purpose to examine it; and I could not but greatly admire the richness and beauty of the collection, espe-

cially that of the fossil bones from St. Peter's mountain; but as the heirs did not consider the expences necessary to transport the collection down the Maese, where each sovereign puts an enormous duty upon every thing that passes through his territories, nor the small number of persons who were likely to purchase it, they rated the price so high that nobody chose to bid for it.

The eldest daughter having at length become possessed of the whole, offered me the principal specimens at a price I agreed to. Amongst them were the duplicates I have already sent to the British Museum, and with which the honourable Trustees are perfectly satisfied. These specimens may serve likewise to ascertain what I have said about them, as being real fragments of physeteres, some of turtles, and the like, but not a single one of any species of crocodile.

§ 3. The arguments for their being jaw-bones and vertebræ of fishes seem to be, first, the smoothness of these bones; and, secondly, the many holes by which the nerves go out at the side, and under each tooth, as is very evident in that beautiful specimen now in the British Museum, on the outside of which eleven holes are visible, in the same manner as they are in the delphini, and more particularly in the lower jaw-bone of the cete, the *Physeter macrocephalus*, or pot-fish, cachalot, &c. Thirdly, the form of the teeth, which have solid roots, as in tab. XV. fig. 6. B, C, E, F, and the six teeth of tab. XVI. Fourthly, because there are little teeth in the palate, as in Dean GODDING's specimen. Fifthly, because the vertebræ have the appearance of true cetaceous vertebræ, as in fig. 5. tab. XV. and in several beautiful and large specimens now in the Museum. Several of these vertebræ were besides intirely unknown

known to me, and not at all analogous to the vertebræ of the crocodile, described and represented by Dr. N. GREW.

§ 4. As I intended to visit London in 1785, I flattered myself I should still find the skeleton of the great crocodile formerly at Gresham College, and be able to find out such characteristic distinctions as should be necessary to decide the question. Dr. GRAY was so kind as to go with me to the lower apartments of the British Museum, where we found, though not without difficulty, the skeleton much neglected, spoiled, and deprived of several interesting parts. I admired, nevertheless, the remainder of it, being infinitely pleased with the transverse sutures, tab. XV. fig. 1, 2. *a, b, c, f, d, e* by which not only those of the neck and thorax, but those of the loins also, are divided, and which I made a drawing of, as large as the life, the 20th of October, 1785, of which fig. 1. and 2. are very accurate copies.

I confess I had not observed that particular division or suture in the skeleton of a small crocodile, of thirteen inches, made by my youngest son; but after being apprized of it by the large skeleton in the Museum, of twelve feet four inches, Paris measure, on looking at my own when I returned home, I found them both alike, and that those parts were not epiphyses; of which, however, the transverse processes of the neck. fig. 1. *d, e, g, o, n, p*, have all the appearance, though there is no other epiphysis to be observed in the rest of the bones of that large skeleton.

When we compare the fossil vertebra, fig. 5. with those now in the Museum, we shall find the epiphyses AB, CD, analogous to *a, b, c, d*, fig. 4. being the real epiphyses in the vertebra of a young porpoise.

I procured,

I procured, in London, the largest vertebræ of the neck of a turtle I could get, and prepared two of them as in fig. 3. in which, as along the back of that singular creature, I found the transverse divisions *a, c, d, f*: of all which I have not seen a single instance amongst the dorsal spinæ from St. Peter's mountain, one of which consists of seven, another of twelve, and a third of fourteen vertebræ. Some of the vertebræ have, I acknowledge, an inferior process, as in the crocodile, *l, m*, fig. 1. Of these I have sent likewise two to the Museum. The ostrich, and the turtle *Mydas*, have such processes, but no quadruped I know of.

The articulation of the vertebræ with each other, by the surfaces of the bodies themselves, is intirely different, not only from that of the crocodile, but from that of all the cetaceous fishes I have ever seen: and I dare venture to assert, I have seen a great many, exclusive of those in my collection. The anterior part of the Maestricht vertebræ is more or less triangular and hollow, as in fig. 5. C, D, L. The posterior A B is convex. Both these surfaces are very smooth, as if they had been covered with a very thin cartilage, and moved one upon the other, without being united by an elastic lamella, as in all quadrupeds and cetaceous fishes; in which the vertebræ have on both the surfaces a round brim, or circular edge, *a, b, i, b*, by means of which the ligaments are connected, and a flat hollow surface within, as *b, i*, fig. 4. for the elastic pulp that is between them.

§ 5. The dentition is so singular in these fossil jaw-bones that it deserves a particular description. In all quadrupeds, as in man, the teeth which appear first are all shed at a certain period of life, and in the mean time new ones are formed above, under, or at the sides of the primordial or temporary teeth,

teeth, but in different sockets. The grinders are not all renewed, but in general three when there are six, and two when there are five. Nature, however, is not always uniform in this operation. Mr. JOHN HUNTER, a worthy Member of our Society, has given a very interesting and complete natural history of the teeth, in which these observations are stated.

In the crocodile the succeeding or secondary teeth appear even when the animal's head is equal to two feet; that is, when it has acquired one-third of its usual growth. When they grow too fast, before the temporary tooth is shed, they perforate the side of the bone, at the part where they meet with the least resistance. Instances of this variety occur in the large crocodile's head, which is in my collection.

In all quadrupeds the enamel is, of the solid parts of the teeth, the first formed, making a cavity, in which the other bony substance is deposited, and formed by lamellæ placed one within another, as is observed by Mr. JOHN HUNTER in the work already mentioned, p. 92. To this the root is added, which is filled in the same manner till the tooth is long enough to pierce through the gums.

But in the fossil jaw-bones of St. Peter's mountain, a small secondary tooth is formed, with its enamel and solid root at once, within the bony substance of the primordial or temporary tooth itself, as is to be seen in the small fragment now in the British Museum, and in tab. XVI. A, B, C, D, E; which, by continuing to grow, seem to make by degrees sufficient cavities in the bony roots of the primary teeth: but what becomes of them at last, and how they are shed, I am not able to guess. I have one in my collection, where the succeeding tooth is intirely formed within the center and substance of the primordial tooth. In the 6th figure (tab. XV.) a little oval cavity is observable,

observable, which has been the feat of a new or secondary tooth.

§ 6. The maxilla inferior of the incognitum, sent by me to the British Museum, is a most magnificent specimen, having fourteen teeth. A similar one, somewhat longer (as it measures $3\frac{1}{2}$ feet) in my own collection, has also fourteen. Another fragment of the left side, two feet long and eight inches broad, shews the primordial and succeeding teeth in the clearest manner.

The specimen, of which I sent a drawing (tab. XVI.) to the illustrious President of our Society, Sir JOSEPH BANKS, is still more useful to confirm the mode of dentition than any other I have in my museum.

§ 7. Several ribs and the phalanges of the toes of the fore-feet, a specimen of which I sent in a fragment from the same rock, of about a foot long and eight inches broad, may serve as another proof of the difference between these and the crocodile's toes, when compared with the still valuable, though neglected, skeleton in the British Museum; which I am sorry I could not make a drawing of, having been too much employed on other objects.

All these characteristic differences cannot fail to convince the learned Society of the truth of what I have asserted, about the animal these bones belonged to; for though we cannot determine exactly the species itself, yet I flatter myself the preceding observations evidently prove, that they did not belong to any animal of the crocodile kind.

▲ § 8. Another very beautiful specimen, a foot and a half long, and about ten inches broad, I have been induced to add, because it contains the anterior part of the scutum of a very large turtle. Of this Mr. JOHN HUNTER has an analogous bone

bone from the same mountain in his valuable collection, but sent to him under another name. I am convinced it belonged formerly to a turtle; first, because I have from the same mountain the intire back of a turtle, four feet long and sixteen inches broad, a little damaged at the sides, and a pretty large fragment of another turtle, in my possession. 2dly, Because I have a similar one, but so placed within the matrix as to shew the inside, which is perfectly analogous to the inside of that piece in the back of a large turtle I got in London, by the favour of Mr. SHELDON. 3dly, Because I have amongst these bones the lower jaw-bone of a very large turtle, of which the crura, though not intire, are seven inches long, and distant from one another six inches; the thickness is equal to $1\frac{1}{4}$ inch.

All these fragments prove the frequency of turtle bones amongst the other fossil bones found in the mountain near Maestricht.

Dr. MICHAELIS wrote to me some time ago, that the above-mentioned fragment, in Mr. J. HUNTER's Collection, belonged to a bird; which I could hardly believe, as I never had seen in any collection whatsoever, either in London, Paris, Brussels, Gottingen, Cassel, Brunswic, Hanover, or Berlin, nor in my own country, any fossil bone belonging to a bird. I know there is a small one described in the Abbé ROZIER's *Journal de Physique*, for March 1782, which is at present in the collection of M. D'ARCET, at Paris. I expect also from Montmartre a small leg of a petrified bird; but these are the only ones I have ever heard of, those of Stonefield, near Woodstock, being most undoubtedly of fishes. I think it is a circumstance worthy the attention of the curious, that no human

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bones, and of birds but very few, have been hitherto found in
a petrified state, and belonging to the old world.

PETRUS CAMPER.

Klein Lankum, near Franeker,
June 18, 1786.

EXPLANATION OF THE PLATES.

T A B. XV.

Fig. 1, 2. Are vertebræ taken from the skeleton of the crocodile described by Dr. NĚH. GREW, in his Catalogue of the Natural Rarities at Gresham College, p. 42. and p. 43.

a, b, c, f, δ, ζ. the bodies of the vertebræ; *a, b* of the fourth; *c, f* of the first vertebra of the neck; *β, x, t.* and *x, y, w.* the spinous processes; *γ β.* and *s.* the ascending; *t,* and *u v.* the descending processes; *g, h, c, i. d, e, n, p, o, q.* the transverse, united by cartilages to the bodies of the vertebræ. GREW calls them *offa mucronata*. The transverse processes of the fourth vertebra being lost, the roots of the mucronated processes are very evident at *g h, i k.*

On the under part of these vertebræ are (*l* and *m*) processes, similar to those we find in the vertebræ of the neck in turtles and birds. Not only the six posterior but the five anterior vertebræ of the back are provided with such processes; of these, however, Dr. GREW makes no mention.

Fig. 2. Represents the seventh vertebra of the back; A, and C. are the ascending and descending processes, forming the articulations with the adjacent vertebræ; B. the transverse process, to which is united the rib FB. in B.; DE. the spinous process; H, H, I. the body of the same vertebra.

These figures are as large as the life, and made from the same skeleton, now in the British Museum. The whole length is equal to $12\frac{1}{2}$ feet, Paris measure; the head equal to 2 feet; the neck equal to 1 foot; the trunk equal to 3 feet 8 inches; the tail equal to 5 feet 8 inches. The measurement given by Dr. GREW does not agree with mine; but he seems not to have taken it with great attention (p. 42.), for he makes use of the words *about, almost, &c.*

OBSERVATION. What struck me was, the transverse future, *a, b, c, f, d, e, g.* which divided the bodies of all the vertebræ of the neck, back, and loins. This division ended with the os sacrum, which was entire, as were also the vertebræ of the tail. Dr. GREW seems only to have taken notice of the futures belonging to the transverse processes.

I have a small skeleton of a crocodile equal to 13 inches, in which the 7 vertebræ of the neck, 12 of the back, and the 5 of the loins, are divided in the same manner as in the large skeleton in the British Museum. Those of the os sacrum and tail are without, and have no mark of an epiphysis.

CONCLUSION. The transverse division of the vertebræ above-mentioned is also peculiar to this animal; and there is no epiphysis, as in other animals.

To be sure of this, I dissected and made a skeleton of the *Lacerta Iguana*, LINN. sp. 26. perfectly well described by

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MARCGRAF, *Hist. Bras.* p. 236. cap. 11.; but I found no such divisions, though the animal was young, and though it had still epiphyses on the legs, &c. The neck consists of 4 vertebræ, the back of 11, the loins of 9, the os sacrum of 2, as in the crocodile; the tail of more than 60.

The dissection of tortoises seemed to me of consequence, at least a more accurate inspection of the vertebræ, particularly those of the neck, as being analogous in some respects to those of the crocodile, especially in the structure of the inferior processes D, and E, with *l, m*, fig. 1.

Fig. 3. Represents two vertebræ of the neck of a pretty large turtle, natural size.

AB, BC. the bodies; L. and I. the ascending, H. and T. the descending processes; R. K. the spinous, *a, b, d, e*. the transverse, and D. E. the inferior processes.

a, b, c, d, e, f. the transverse division of these, similar to that in the crocodile.

Fig. 4. A vertebra from the tail of a young phocæna or porpoise; in which *a, b*. is an orbicular plate, united by means of cartilage to the body of the vertebra *a, d*. which is provided with such a one on both sides, *a, b*. and *c, d*.

Those bony lamellæ are the epiphyses of the vertebræ, and are alike in all quadrupeds, to which class all the cetaceous fishes belong. When we consider the structure in general of these last, we find the hind legs only are wanting, and of course the ossa innominata; but the ossa pubis are very remarkable in all of them.

Fig. 5. Is a fossil vertebra of the unknown animal, whose bones are so often met with in St. Peter's Mountain at Maestricht,

tricht. A, B, C, D. is the body; C, I, K, E, F. the spinous processes; C, K, I. the medullary canal, running under K, E, F, in a direction parallel to IF, and coming out again at F. The remaining marks of the lamellated epiphyses I, D. and A, B. are evident proofs of the analogy between these and the vertebræ of the cetaceous fishes; and also of their want of resemblance to the vertebræ of the crocodile, as will appear by comparing the first and second figures with the fifth.

Fig. 6. Is a very accurate drawing of one of the fossil teeth belonging to the same incognitum. A B C. is its point, of a lanceolated figure, whose edges B A, and A C, are dentated; B C. is the root, uneven, bony, fixed within the socket with D, G, F.; D, G, B, C. is covered with the gums; H, I. is an oval sinuosity, in which generally the secondary teeth are generated, as is seen in tab. XVI. representing a fragment of the upper jaw-bone of the same incognitum, A, B, C, D, E.

The teeth in all the *Phyfeteres* and *Delphini* have solid roots, except in the young ones, in which they often have cavities to receive the blood-vessels and nerves. But the crocodile has the teeth intirely hollow, as appears in

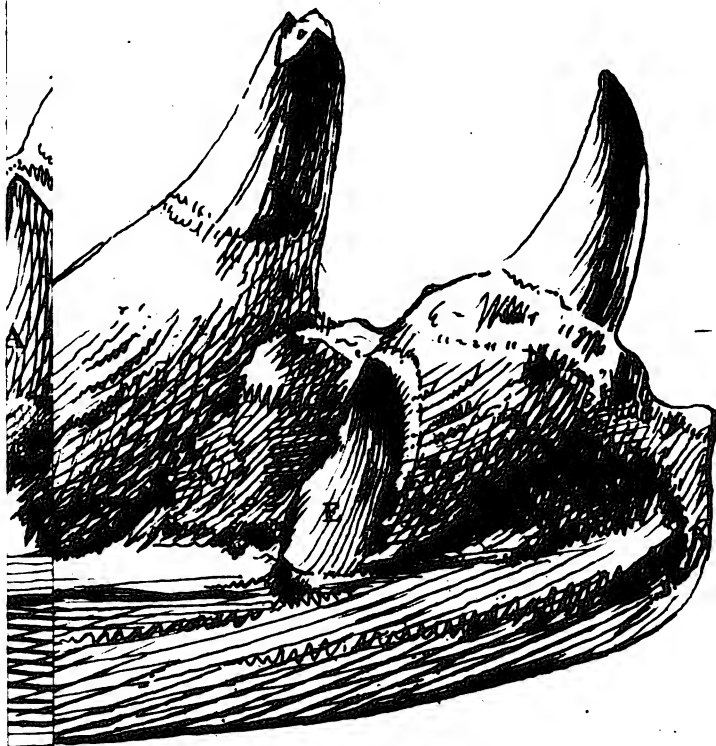
Fig. 7. in which the cavity Π , Δ , Θ , shews the difference between the crocodile's teeth and those of the cetaceous and other fishes. This tooth is the anterior one of a large head of a crocodile, two feet long, and of the same size as that in the British Museum. A hollow tooth may notwithstanding belong to a *Phyfeter*, as Dr. OTHO FABRICIUS observes in his *Fauna Groenlandica*, p. 44. when speaking of the *Phyfeter microps*: of which he says, "*Habet in maxilla inferiori dentes*

22, utrinque 11 arcuatos, falciformes, *intus ad apicem usque cavos*," within they are hollow to the very end.

T A B. XVI.

Fragmentum Maxillæ superioris, lateris dextri capitis Physteris incogniti, ex Monte St. Petri, Traj. ad Mosam. Origo dentium serotinorum ex ipsis radicibus solidis primo enatorum in quinque manifesta est. Quæ ad dentitionem hanc singularem pertinent, ex figur. 2. Tab. Fragm. similis sed Maxill. inf. 12 Aug. 1784. peti. debent.

*Testudinis . Marinae
Vertebrae e Collo.
et .*



P. Camper f.
Lancumi 27 Maji
1785.
magnit. natur.

XXVII. *Catalogue of One Thousand new Nebulæ and Clusters of Stars.* By William Herschel, LL.D. F. R. S.

Read April 27, 1786.

THE following Catalogue, which contains one thousand new Nebulæ and Clusters of stars, is extracted from a series of observations (or Sweeps of the heavens), which was begun in the year 1783, and which I am still continuing till the whole be completed. As I may, perhaps, find an opportunity hereafter to publish these observations at full length, I shall now only mention such circumstances, relating to the instrument and apparatus with which they were made, as will be necessary to shew what degree of accuracy may be expected in the determination of the places of these Nebulæ and Clusters of stars; and also to serve any astronomer, who wishes to review them, to form a judgment what instrument will suffice for this purpose.

The telescope I have used, as has been observed on a former occasion*, is a Newtonian reflector of 20-feet focal length, and $18\frac{7}{8}$ inches aperture. The sweeping power has been 157, except where another is expressly mentioned. The field of view $15' 4''$.

My eye-glass is mounted on that side of an octagon tube, which, in the horizontal position of the instrument, makes an angle of 45° with the vertical; having found, by experience, that this position, resembling the situation of a reading desk, is

* Philosophical Transactions, vol. LXXIV. p. 437.

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preferable to the perpendicular one commonly used in the Newtonian construction.

In the present improved state of the apparatus this telescope will, in general, give the relative place of an object by a single observation true to within $1\frac{1}{2}$ or 2 minutes of polar distance, and 4 or 6 seconds of time in right ascension. But when there is an opportunity of repeating the observation, it will hardly differ a single minute in the former, and seldom so much as 3 or 4" in the latter. My apparatus, however, has not been equally perfect from the beginning; for, being from time to time adapted to the different views I had in sweeping, it could only arrive to its present degree of perfection by many experiments and gradual improvements.

To begin a short history of this 20-feet telescope. In the month of October of the already mentioned year I began to use it, being then mounted on its present stand, but with a lateral motion under the point of support of the great speculum, by which its direction could be changed about 15 degrees. It had also a kind of moveable gallery in front, about nine feet long, which permitted me to follow a celestial object near 15 degrees more; by which means I obtained a range of 30 degrees without moving the stand. The Newtonian form has the capital advantage of rendering observations equally commodious in all altitudes; I had therefore placed the instrument in the meridian, that I might view the stars in their most favourable situation.

When I had seen most of the objects I wished to examine, I proceeded to the work of a general review of the heavens. The first method that occurred was, to suffer the telescope to hang freely in the center; then, walking backwards and forwards on the moveable gallery, I drew the instrument from
that

that position by a handle fastened to a place near the eye-glass, so as to make it follow me, and perform a kind of very slow oscillations of 12 or 14 degrees in breadth, each taking up generally from 4 to 5 minutes of time. At the end of each oscillation I made a short memorandum of the objects I chanced to see; and when a new nebula or cluster of stars came in my way, I made a delineation of the stars in the field of view, both of the finder and of the telescope, that it might serve me to find them again. This being done, the instrument was, by means of a fine motion under my hands, either lowered or raised about 8 or 10 minutes, and another oscillation was then performed like the first. Thus I continued generally for about 10, 20, or 30 oscillations, according as circumstances would permit; and the whole of it was then called a *Sweep*, and as such numbered and registered in my journal.

When I had completed 41 Sweeps, the disadvantages of this method were too evident to proceed any longer. By going into the light so often as was necessary to write down my observations, the eye could never return soon enough to that full dilatation of the iris which is absolutely required for delicate observations. The difficulty also of keeping a proper memorandum of the parts of the heavens which had been examined in so irregular a manner, intermixed with many short and long stops while I was writing, as well as the fatigue attending the motion, upon a not very convenient gallery, with a telescope in my hands of no little weight, especially at the extremes of the oscillations, where it made a considerable arch upwards, were sufficient motives to induce me to look out for another method of sweeping. And it is evident, that the places of nebulae hitherto determined, which was till the 13th of December, 1783, must be liable to great inaccuracy. I therefore

began now to sweep with a vertical motion; and as this increased the labour of continually elevating and depressing the telescope by hand, I called in the assistance of a workman to do that part of the business, by which means I could observe very commodiously, and for a much longer time than before.

Soon after I removed also the only then remaining obstacle to seeing well, by having recourse to an assistant, whose care it was to write down, and at the same time loudly to repeat after me, every thing I required to be written down. In this manner all the descriptions of nebulae and other observations were recorded; by which I obtained the singular advantage that the descriptions were actually writing and repeating to me while I had the object before my eye, and could at pleasure correct them, whenever they disagreed with the picture before me without looking from it.

In about half a dozen sweeps, done according to this new way, I found that the stars of FLAMSTEED's Catalogue entered nearly at the time when they were expected; this suggested the possibility of converting my telescope into a transit instrument. By way of trial, Dec. 18, 1783, I began to use a watch, and noted the times of the transits of stars and nebulae to the nearest minute; and, this succeeding, Dec. 24, a sidereal time-piece was introduced.

I found also that, by the turns of the handle which gave motion to the telescope, it was practicable, in a coarse way, to ascertain the difference of altitude between any two objects that passed the field of view; on which account, Dec. 30, I began to use an index-board, divided into inches, and marked with numbers, which, being placed behind the rope that moved the telescope, would point out at what altitude a certain index, affixed to the rope, was situated. My tackle of ropes and pullics

pullies was such that, while the telescope traversed an arch of two degrees, the mark on the rope passed over about 24 inches of the index-board : but the exact measure was always to be determined experimentally, as it varied according to the situation of the instrument. I perceived immediately that the quantity of rope used in the motion of the telescope would be much better observed by the assistant, if the index were brought within doors near the writing desk : to effect this, I used a small cord, which, being led off from the great one, was carried over a pulley into the observatory, so as to pass over a set of numbers, which I now divided into such parts as, in an equatorial situation of the instrument, would give nearly each equal to one minute.

It would exceed the limits of this Paper to enumerate the various trials I made to bring the right ascension to greater perfection ; such as causing the tube sometimes to hang inclining or rubbing against a perpendicular plane ; at others, drawing it against the same by a small weight, fastened to a cord, passing over a side pulley, &c. I shall also pass over the several changes in the form of the machine shewing the polar distance, which, for convenience sake, was soon brought to an index moving over a dial, in the manner of a clock.

By way of directing the person who gives motion to the telescope, a small machinery was added, which strikes a bell at each extreme of the breadth of the sweep, and is adjustable to any required number of turns of the handle.

In June, 1784, I introduced a small quadrant of altitude, the use of which became soon after of the greatest consequence in determining the value of the numbers of the polar distance piece. Hitherto I had settled this value by causing a star to pass vertically through the field of the finder, which was very

accurately limited to two degrees; but now I found, by many comparisons between the degree determined by the quadrant and by the finder, that I had generally under-rated the value of the numbers. Fortunately so many stars of FLAMSTEED's Catalogue had been taken, that the numbers between their different polar distances were sufficient to recover the value of the degree; but this occasioned a laborious re-calculation of the places of all objects taken in near 300 sweeps. The quadrant being once introduced, I carried the refinements of the determination, in high sweeps where the ropes acted very unequally, so far as to ascertain by it separately the value of every 20 or 30 minutes throughout the whole breadth of a sweep of two degrees, and the numbers were then accordingly cast up by so many different tables calculated on purpose.

Being still disappointed in many instances, when, on a review of a nebula whose place I had before determined, I perceived a difference of 4 or 5 minutes in polar distance, I began at last intirely to new model the machinery of the polar distance piece, and on Sept. 24, 1785, completed one with the following capital improvements. My former piece shewed a set of numbers whose value differed in every situation of the telescope, and therefore required different and very extensive tables to cast them up in degrees and minutes. This shews at once both the degree and minute of the polar distance of every celestial object, without requiring any tables to cast up numbers. In the next place, the considerable inaccuracy arising from the unequal tension of the great ropes, and their expansion or contraction by moisture or dryness, is intirely taken away; for now my index cord is contrived so as to go off from the front of the telescope itself, in the direction of a tangent to the arch it describes when moving; by which means this cord will even serve

serve as an hygrometer to shew the variations of the ropes that suspend the telescope. If a shower of rain, for instance, should shorten them so as to elevate the telescope 2, 4, or 6 minutes, which has happened sometimes, notwithstanding they have all been well saturated with oil, the index cord will immediately make the polar-distance-clock shew this effect of the rain, by pointing out an equal change on the dial. As to the variations of the cord itself, they are in the first place very trifling, since it consists merely of a few threads of hemp, very loosely twisted, well oiled, and always equally stretched; but especially these variations are of no consequence, as they are so easily to be discovered by the check of the quadrant of altitude affixed to the telescope, or the successive transits of known stars, and may either be immediately corrected by the adjustable hand of the polar distance dial, or be left to be accounted for afterwards.

The improvement of the right ascension has not been less attended to; and the Royal Society having kindly intrusted me with an excellent time-piece, I succeeded at last by means of the addition of the following apparatus. Against the side of the tube is fixed a vertical iron plate, and the point of suspension of the telescope is disposed so as to permit this plate to be just in contact with a roller which remains fixed during the time of a sweep. There is also a considerable spring applied on the opposite side, in such a manner as, by always exerting a pressure nearly uniform, to cause the iron plate to rub against the fixed roller as the telescope sweeps up and down. By this means I have frequently, in very stormy weather, observed many hours without finding my time materially affected, and the corrections will seldom, in accurate observations, exceed a few seconds.

To those who are accustomed to the accuracy of transit instruments in regular observatories, this telescope, notwithstanding the above-mentioned improvements, may perhaps appear far from being brought to perfection; but they should recollect the size of the instrument as well as its extensive use, since I can not only follow any object for near a quarter of an hour, without disturbing the situation of the apparatus, but can at pleasure, in a few minutes, turn it to any part of the heavens, and view a celestial object wheresoever it may chance to be situated, even the zenith not excepted.

From this account it will be understood, that the places of a few of the nebulae and clusters of stars, determined before the 13th of December, 1783, may be faulty in right ascension as far as 1' of time, and in polar distance to 8 or 10' of space. Afterwards the errors will be found to become gradually less considerable till the latter end of the year 1784, when, I suppose, they will seldom exceed half that quantity. From that period to Sept. 24, 1785, they will diminish, and probably not often amount to so much as 3 or 4' in polar distance, and 10 or 12'' in right ascension. And now I flatter myself that all places, determined since the last mentioned time, will generally be true to a very small quantity; such as 4 or 6'' in right ascension, and 1½ or 2' in polar distance, and often much nearer.

Some of the nebulae in that part of the heavens which, in a former Paper, I have called the stratum of Coma Berenices, are indeed so crowded that there was no possibility of taking them all in the center of the field of view, and a somewhat less degree of accuracy may therefore be expected; but having used myself by very frequent estimations of the parts of the field of view to judge of their value in time as well as in space,

I corrected

I corrected this defect at the moment of observation by affixing to the transits of these excentric nebulæ such proper marks of *plus* or *minus* in right ascension and polar distance as I judged would bring them to a central observation. A similar method, well known to good astronomers in estimating their tenths of seconds by the proportional space over which the stars move in their meridian passage, makes it unnecessary to expatiate on the degree of accuracy that long practice enables us herein to obtain.

If, however, I had been willing to delay giving this catalogue till, by a repeated review of the heavens, the places had been more accurately determined, the work would undoubtedly have been more perfect; but whoever considers that it requires years to go through such observations will perhaps think with me, that it is the best way to give them in their present state, if it were but to announce the existence of such objects by way of inducing other astronomers also to look out for them. Another motive for not delaying this communication is to shew that my late endeavours to delineate the construction of the heavens have been guided by a careful inspection of them; and, probably, a catalogue which points out no less than one thousand instances of such systems as those are into which I have shewn the heavens to be divided, will considerably support what has been said on this subject in my two last Papers.

When the diurnal motion of the earth was first maintained, it could not but greatly add to the reception of this opinion when the telescope exposed to our view Jupiter, Mars, and Venus, revolving on their axes*; and if these instances of

* To these may now also be added Saturn, on whose body I have, in the year 1780, seen several belts, with spots that changed their situation in the course of a few nights.

the similar condition of other planets support the doctrine of the diurnal motion, the view of so many sidereal systems, some of which we may discern to be of a most surprising extent and grandeur, will in like manner add credit to what I have proposed with regard to the condition of our situation within a system of stars: for, to the inhabitants of the nebulae of the present catalogue, our sidereal system must appear either as a small nebulous patch; an extended streak of milky light; a large resolvable nebula; a very compressed cluster of minute stars hardly discernible; or as an immense collection of large scattered stars of various sizes. And either of these appearances will take place with them according as their own situation is more or less remote from ours.

In the distribution of the nebulae and clusters of stars into classes, I have partly considered the convenience of other observers: thus, in the first class, the degree of brightness of the nebulae has been the leading feature, as most likely to point out those which their several instruments may give them expectation to reach. The first class, therefore, contains the brightest of them; the second, those that shine but with a feeble light; and in the third are placed all the very faint ones. Besides this general division, I have added a fourth and a fifth class, which contain nebulae that, on different accounts, seemed to deserve a more particular description than I had allotted to the three former divisions.

The clusters of stars are sorted by their apparent compression, in the manner of my former Catalogues of double, treble, and multiple stars; so that the closest and richest clusters take up the first class; the brightest, largest, and pretty much compressed ones, the second; and those, which consist only of scattered and less collected large stars, are put into the last.

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In every class the order of time when the nebulæ and clusters of stars were discovered, or first observed with my 20-foot telescope, has been followed; and that I might describe all these objects in as small a compass as could well be done, I have used single letters to express whole words, an explanation of which, with an example of the manner of reading those letters, is given. It should be observed, that all estimations of brightness and size must be referred to the instrument with which the nebulæ and clusters of stars were seen; the clearness and transparency of the atmosphere, the degree of attention, and many more particular circumstances, should also be taken into consideration; so that probably some of the nebulæ which I have called very bright, and very large, may only be just perceivable, as very small faint patches, in many of our best common telescopes.

The Identity of each nebula in this catalogue has been well ascertained by a projection on a proper map, made on purpose, which pointed out all other nebulæ near its place, and thus afforded the means of a rigorous examination. When, therefore, several nebulæ are found within the limits of the accuracy with which my telescope can discriminate them, in different nights, it may be concluded, that they were seen either at once in the same field of view, or otherwise in immediate succession during the same sweep.

In the same manner these nebulæ have been compared with those that are contained in the two volumes of the *Connoissance des Temps*, for the years 1783 and 1784, of which none have been inserted in this catalogue. It was indeed easy enough to distinguish the nebulæ of that excellent collection from those of mine which in several places are very near them: The quantity of good light in my telescope having enabled me,

even in bright moon-light nights, to see occasionally some of the most feeble of the former, when the latter could not by any means be perceived.

Perhaps it will not be displeasing to those who may look out for some of the objects contained in this catalogue, to know that the pictures which were given in a former Paper, representing the various shapes and appearances of several nebulae, have been actually taken from nature, by Drawings made of them while I had them in view; I have therefore added a reference to these figures, as the descriptions of the originals which they represent occur in their order in the catalogue.

Arrangement of the columns, and explanations of the abbreviations.

The first column contains the class and number of the nebulae.

In the second are the dates when the nebulae were first observed.

The third column contains the star, or other object, by which the place has been determined.

In the fourth column the letter p or f shews that the nebula is either preceding or following the star.

In the fifth is the time, in sidereal minutes and seconds, by how much it precedes or follows the same star.

The letter n or s, contained in the sixth column, denotes that the nebula is north or south of the determining star.

In the seventh is the quantity, in degrees and minutes, by how much the nebula is more north or more south than the same star.

The eighth column contains the number of observations that have been made of each nebula; and it is to be noted, that

the determination of the place is generally taken from the last observation, on account of the more perfect state of the telescope.

The ninth column, or remaining space, contains the description of the nebulae, by means of single letters, or now and then a few words added to them.

The abbreviations are to be understood as follows.

B. Bright.	v. very.
F. Faint.	c. considerably.
L. Large.	p. pretty.
S. Small.	e. extremely.

Of these letters I have composed vB. cB. pB. pF. vF. eF. vL. pL. pS. vS. eS.; all which require no farther explanation.

R. Round.	l. a little.
E. Extended.	i. irregularly.
M. in the middle.	g. gradually.
b. brighter.	f. suddenly.
m. much.	

When these are joined we have iR. mE. lE. bM. gbM. fbM. mbM. lbM. gbmM. gmbM. fmbM., and by taking in some of the former letters BM. vBM. cBM.; where no other remark will be necessary than that writing for instance bM, or brighter in the middle, it is intended to express, that a nebula, which is faint at the borders, is less so towards the middle. And these degrees of brightness happening sometimes to be so well united from the most imperceptible border to a very luminous center, I have, on such occasions, used the expression vgmbM, or very gradually much brighter in the middle.

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| r. resolvable. | m. milky. |
| er. (joined) easily resolvable. | |
| iF. (joined) of an irregular figure. | |
| C. Cometic, or resembling a telescopic comet. | |

N. having a Nucleus, or bright compressed spot.

l, b, or d. (joined to minutes) long, broad, or diameter.

ft. a star. stars.

n. north. north of.

s. south. south of.

p. preceding. np. north preceding. sp. south preceding.

f. following. nf. north following. sf. south following.

betw. between. ver. 240. verified by a power of 240.

bran. branches.

che. chevelure.

mer. in the direction of the meridian.

par. in the direction of the parallel of declination.

np sf. in a direction from north preceding to south following.

sp nf. in a direction from south preceding to north following.

Example. I. 13. 22. 69 Leon. p. 7. 57. n. o. 2. 3. vB. mE.
mer. fmbM. 7 or 8' l.

13th nebula of the 1st class. Feb. 22, 1784. It precedes the 69th Leonis of FLAMSTEED's Catalogue $7' 57''$ in time, and is $0^{\circ} 2'$ more north than that star. 3 observations. Very bright, much extended in the direction of the meridian of the nebula, suddenly much brighter in the middle 7 or 8' in length.

I. 32. p. 5. 11. n. o. 28. 3. cB. S. BN. and 2vF bran.
32d nebula of the first class. April 13, 1784. It precedes the 31st (or 1st d) Virginis of FL. Cat. $5' 11''$ in time, and is $0^{\circ} 28'$ more north than that star. 3 observations. Considerably bright, small, having a bright nucleus, and two very faint branches.

First class. Bright nebulæ.

I.	1783	Stars.		M. S.		D.M.	Or.	Description.
1	Dec. 19	82 (3) Ceti	f	2 17	n	0 8	7	cB. cL. iF. bM.
2	—	3 Leonis	p	18 7	f	1 12	5	cB. cL. vgbM. N. R.
3	—	34 Sextant	p	28 55	f	0 13	4	cB. pL. C. mbM.
4	—	—	p	28 27	f	0 10	4	cB. pL. C. mbM.
5	30	81 Leonis	p	2 42	n	0 7	2	B. pS. iR. bM. r.
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6	Jan. 19	64 Virginis	f	33 56	f	0 1	3	vB. pL. gmbM.
7	23	49 Leonis	f	126 45	f	0 40	1	vB. L. R. The place inac.
8	—	32 (2) Virg	f	2 50	n	0 48	5	cB. pL. iR. mbM. r.
9	24	10 (r) Virg	f	3 12	f	0 35	4	cB. E. np ff. N and 2 bran. 3'.
10	—	—	f	33 37	n	0 4	4	vB. pL. lE. gmbM. 2' l. 1½' b.
11	Feb. 15	5 Comæ Be.	p	1 30	f	2 11	1	B. pL. lE. bM. m.
12	19	6 Comæ	f	9 12	f	0 9	2	B. pS. R. BM. r.
13	22	69 Leonis	p	7 57	n	0 2	3	{ vB. mE. mer. fmbM. 7 or 8' l. Fig. 11.
14	—	29 (γ) Virg	f	0 43	n	1 23	2	cB. cL. mE. near par. 3 or 4' l.
15	—	—	f	3 23	n	0 58	2	cB. mE. sp af. fbM. 4 or 5' l.
16	—	—	f	10 34	n	0 13	2	cB. vL. iF. vgbM.
17	Mar. 11	46 (i) Leo	{	f 15 50	f 1 32	5	{	The 2 p of 3. Both vB. cL. mbM.
			{	f 16 18	f 1 29	5	{	C. II. 41. Fig. 4.
		11 Comæ	p	10 30	n	0 46	1	vB. pL. gbM.
		73 (n) Leonis	f	8 52	f	1 57	2	vB. mE. nearly par.
		—	f	25 31	f	1 49	3	vB. cL. R. gmbM.
		34 Virginis	p	22 24	f	0 17	2	cB. pS.
		—	p	18 24	f	0 19	2	B. S. mE.
		30 (ε) Virg	p	1 42	f	0 5	2	vB. pL. r. near 2 Bf.
		34 Virginis	f	4 45	f	0 40	1	B. S. in a line with 2 ft.
		52 (K) Leonis	p	3 45	f	2 9	1	cB. pL. not R. mbM.
	Apr. 8	46 (i) Leonis	f	18 47	f	0 43	3	vB. BNM. and 2 F bran. np ff.
	—	34 Virginis	p	19 36	n	1 8	2	{ One of two, at 4 or 5' dist. B. cL.
	12	73 (n) Leonis	p	1 9	f	0 30	3	vB. cL. E. par. mbM.
	13	31 (1 d) Virg	p	17 41	n	0 32	2	vB. cL. lE. iF.
	—	31 (1 d) Virg	p	8 0	n	0 37	1	vB. E. mbM. r. betw. 2 Bf.
	—	—	p	5 11	n	0 28	4	cB. S. BN. and 2 vF. bran.
	15	9 (o) Virgin	f	3 12	n	1 39	1	B. L. mE. mbM. r.
	—	59 (r) Virgin	f	20 42	f	0 34	2	vB. cL. E. np ff. SBN.
	17	34 Virginis	p	31 42	n	1 5	1	B. vmE. vBM. 9 or 10' l.
	18	—	p	11 24	n	0 20	1	{ Two. Both B.
		32 (2 d) Virg	p	11 36	n	0 0	1	{ S. lE. B. vL. mE. mbM.

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I.	1785	Stars.		M. S.		D.M.	Ob.	Description.
82	Apr. 6	14 (b) Comæ	p	37 40	f	0 14	2	cB. pL. lE. mer. vgbM.
83	—	21 (g) Comæ	f	0 10	n	1 12	1	cB. pl, iR mbM.
84	—	—	f	19 34	n	0 55	1	cB. iR. f BM. m. 7 or 8' d.
85	10	40 Comæ	f	5 9	n	0 18	1	cB. pL.
86	11	39 Leonis min	p	13 14	n	0 59	1	cB. pL. mbM. brightness lE.
87	—	44 Leonis min	f	9 30	n	1 1	1	vB. vL. gbM.
88	—	—	f	13 30	n	0 1	1	cB. cL. iR. mbM.
89	—	14 (b) Comæ	p	8 18	n	0 55	1	vB. S. lE.
90	—	—	p	6 30	n	1 57	1	The np of 2 cB. pL R. II. 377.
91	—	15 (c) Comæ	f	1 10	n	0 19	1	vB. E. par. pBLN. and 2 bran.
92	—	—	f	9 8	f	0 19	1	{ vB. vL. mE. np ff. 10 or 12'
93	—	31 Comæ	f	2 56	n	1 24	1	{ l. 4 ft. in it. cB. pL.

Second class. Faint nebulae.

II.	1784	Stars.		M. S.	D.M.	Ob.	Description.
157	April 15	20 Virginis	p	3 36	f 1 29	1	F. pL. mE. hM. r.
158	—	31 (1 d) Virg	p	8 38	n 1 51	3	pT. pL. nearly R. r.
159	17	81 Leonis	f	0 36	u 0 24	1	pB. S. bM. almost stellar.
160	—	—	f	1 0	n 0 45	1	cL. R. vgbM.
161	—	90 Leonis	f	5 36	n 0 53	1	F. not S. R. bM.
162	—	34 Virginis	p	51 54	n 0 0	2	not vF. pL. iR. lb. towards f. fide.
163	—	—	p	33 6	n 1 13	1	pS.
164	—	—	p	32 48	n 0 13	1	pS. vmE.
165	—	—	p	32 30	n 1 13	1	F. vmE.
166	—	—	p	27 36	n 0 53	1	pB. vS.
167	}	—	p	21 30	n 0 49	1	{ Two nebulae.
168		—	p	20 30	n 0 40	1	{ The most f. E.
169		—	p	19 42	n 0 49	1	{ S.
170	—	—	p	19 42	n 0 49	1	F.
171	}	—	p	19 6	n 0 20	1	{ Three nebulae.
172		—	p	19 6	n 0 20	1	{ The two first vS.
173		—	p	19 6	n 0 20	1	{ The third S.
174	—	—	p	17 48	n 1 16	1	F.
175	—	—	p	12 36	n 1 9	1	pF. L.
176	—	—	p	3 48	f 0 37	1	F.
177	—	20 Bootis	f	3 30	f 1 42	1	pF. not S. lbM. r.
178	}	—	p	12 6	f 0 7	2	{ Two, very close. Both S. stel-
179		28 (β) Serp	p	12 6	f 0 7	2	{ lar. The f. is largest.
180	22	15 (α) Virg	f	8 59	f 1 18	3	pB. L. iR. er.
181	—	29 (γ) Virg	f	5 18	f 0 58	1	pF. pL. E. r.
182	—	—	f	6 24	f 1 54	1	pF. pL. E. r.
183	24	51 (θ) Virg	p	30 36	n 0 14	1	pB. cL. E. vfmBm.
184	—	—	p	28 30	n 0 26	1	not F. L. lE. lbM. r.
185	—	—	p	11 0	n 0 10	1	F. S. iF. near pBf.
186	25	28 Virginis	f	12 6	n 0 51	1	pF. cL. R. r.
187	—	—	f	12 42	n 0 37	1	pF. pL. r.
188	—	—	f	22 54	n 0 57	1	F. cL. E. r.
189	—	72 (1 d) Virg	p	21 54	f 0 18	1	pB. R. vfmBm. near Bf.
190	—	26 (x) Virg	f	23 44	f 0 6	2	F. pL. iR. lbM. r.
191	May 9	49 (g) Virg	p	4 6	f 0 46	1	pF. pS. R. r. near some Sft.
192	—	18 Libræ	f	10 36	f 0 16	2	pF. pL. lE. mer. nearly.
193	11	100 (λ) Virg	p	59 30	n 0 48	2	The most n. of 3. pB. vS. bM.
194	19	12 (d) Bootis	f	7 42	f 0 2	2	F. pL. R. mbM.
195	21	39 Ophiuch	p	12 54	n 1 42	2	pB. cL. iR. lbM. r.
196	22	54 Hydræ	p	6 42	f 1 2	1	pB. S. nearly R. bM. r.
197	—	51 (e) Ophiu	f	35 36	f 1 13	1	pB. pL. iR. r.
198	24	3 (p) Sagitt	f	18 42	f 0 4	1	pF. not L. crookedly E. cr.
199	June 16	64 (r) Ophiu	f	2 48	n 0 48	1	pB. pL. R. gbM. r.
200	24	10 (γ) Sagitt	p	1 6	n 0 22	1	F. pS. r. unequally B.

II.	1784	Stars.	M. S.	D.M.	Ob.	Description.
201	July 13	18 Sagittarii	p 7 54 f	0 55 1		F. pL. lbM. r.
202	17	12 (φ) Cygni	f 17 36 f	0 53 1		A retolvable nebulous patch of ft.
203	—	65 (ζ) Cygni	p 9 30 f	0 10 2		pB. pL. iE. bM.
204	Aug. 7	24 Sagittarii	p 9 18 n	0 50 1		pB. S. stellar. not verified.
205	—	—	p 1 42 n	0 33 1		pB. cL. iE. bM.
206	Sept. 7	52 (ι) Cygni	f 5 36 n	1 22 1		F. S. crookedly E. r.
207	—	44 (η) Pegasi	p 34 27 n	1 15 1		cL. R. gmbM. er.
208	10	84 (π) Pegasi	p 13 48 n	1 0 1		F. cL. R. vghM. ff. ft.
209	—	34 (ζ) Andri	p 5 57 n	1 12 2		F. pL. iR. equally B. r.
210	11	31 (δ) Andri	f 18 12 f	0 26 1		F. pL. unequally B. near pBft.
211	—	13 Triang	f 5 24 f	0 35 1		F. pL. iE. bM. n. 2 ft.
212	12	53 Pegasi	p 19 42 f	0 15 1		pB. pL. iE. mbM. r. f. 2 ft.
213	—	79 Pegasi	p 2 36 n	0 42 1		F. pL. ER. lbM.
214	—	40 Androm	p 7 18 f	0 15 1		F. E. p. Bft.
215	}	—	—	—	—	{ Three. mer. Nearly equal in size. All. F. vS. R. propor- tion of dist. f to n. 2 to 1.
216		—	f 4 30 n	0 41 1		
217		—	—	—	—	
218	}	—	f 5 30 n	1 22 1		{ Two. The p. F. vS. The f. pL.
219		—	f 7 36 n	1 22 1		
220		—	—	—	—	
221	—	3 (ι) Triang	p 6 12 f	0 15 1		F. pL. mE. r. 1½ l.
222	—	—	p 5 12 f	10 0 1		F. pL. mE. r. 1½ l.
223	—	—	p 2 12 f	1 52 1		pB. pS. R.
224	13	43 (β) Andri	p 0 18 n	0 5 1		pB*. cL. R. bM. { * Though β And. in the field.
225	—	9 (γ) Triang	f 4 18 f	0 39 1		F. vS. R.
226	15	71 (γ) Pegasi	p 4 54 f	0 5 1		F. pL. bM. elliptical.
227	—	89 (χ) Pegasi	p 10 18 n	0 32 2		F. cL. mE. r.
228	}	—	—	—	—	{ Two. Both F. pS. iR.
229		6 (β) Arietis	p 5 12 n	1 7 1		
230		1881 (φ) Pegasi	p 1 27 n	1 4 1		
231	—	—	p 1 3 n	0 59 1		F. pL. E. par. contains a stell. or ft.
232	—	—	f 6 45 n	1 35 1		F. S. R. or large stellar.
233	}	1947 (λ) Pegasi	p 9 3 n	0 12 3		{ Two. The p. pB. iE. nearly mer. The f. F. E. nearly par. 1½ l.
234		—	p 8 33 n	0 14 3		
235		2011 Piscium	p 13 43 f	0 36 2		
236	—	90 (φ) Aqua	f 3 53 n	1 22 4		F. pL. broadly E.
237	—	79 Ceti	p 4 48 n	0 36 1		pB. pL. iR. mbM.
238	Oct. 6	26 (β) Persei	p 28 34 f	0 10 2		F. E. mer. 2' l.
239	—	727 (α) Persei	p 8 27 n	0 2 1		pB. mE. near par. mbM. 4' l 1' b.
240	—	8	—	—	1	The 1st of 2. pB. pS. r.
241	—	—	—	—	1	pF. pL. iR. er.
242	11	48 (μ) Pegasi	p 39 50 f	0 54 2		pS. C.
243	—	—	f 6 27 f	0 54 2		F. S. iR. near and p. 2 or 3 ft.
						F. S. iR.

II.	1784	Stars.		M.	S		D.M.	Ob.	Description.	
244	Oct. 14	54 (α) Pegasi	f	30	48	n	0	6	2	F. S. lE.
245	—	58 Piscium	p	3	36	n	2	16	4	pB. pL. R. lbM.
246	—	19 Arietis	f	4	54	f	0	49	1	F. pL. E. 4 or 5' f. cft.
247	15	13 Pegasi	f	10	0	n	0	28	1	pB. R. bM. 1' d.
248	—	54 (α) Pegasi	p	38	0	n	0	59	2	F. pS. a quartile with 3 Sft.
249	—	—	p	3	36	n	1	11	2	F. pS. E. f. pBft.
250	—	47 Piscium	p	67	12	f	0	37	1	F. lE. p. vBft.
251	16	54 (α) Pegasi	p	4	36	n	0	44	1	pB. cL. E. r.
252	—	102 (π) Pisc	p	12	48	n	0	45	1	F. pL. oval. lbM. p. pBft.
253	—	—	f	5	54	n	1	30	1	pB. pL. E. bM. r.
254	—	38 Arietis	f	8	48	n	0	34	1	F. S. iR. r.
255	18	82 Pegasi	p	8	21	f	0	11	2	pB. pS. R. gbM. r.
256	—	77 Pegasi	f	1	0	f	0	25	1	F. R. gbM.
257	—	34 Piscium	f	12	6	f	0	39	2	F. pL. iR. mbM.
258	20	15 Eridani	p	8	54	n	1	54	3	F. vL. lbM. R. 7 or 8' d.
259	Nov. 16	43 (γ) Cancr	p	20	58	n	1	2	1	F. S. iF.
260	—	4 (λ) Leonis	f	3	22	f	1	16	1	F. pS. lE.
261	17	12 Pegasi	f	2	8	f	0	46	1	F. iR. less than 1' d.
262	—	27 Eridani	p	11	51	f	1	40	1	F. l and iE. above 1' d.
263	—	—	p	9	28	f	1	15	1	not vF. bM. 1½ d.
264	—	47 (δ) Cancr	p	67	42	n	2	20	1	F. S.
265	20	4 (1 χ) Can	p	19	20	n	1	28	1	pF. pS. iF. lE. bM.
266	—	15 (ι) Nav	f	25	33	n	1	25	1	F. E. bM. r. 1½ d.
267	Dec. 9	27 Eridani	p	6	1	n	0	40	1	F. vS. R. lbM.
268	—	8 (ι) Crateris	p	63	16	f	0	16	1	F. S. R. SB point M. C.
269	—	10 Crateris	f	4	26	n	1	22	1	pB. pL. lE. mbM.
270	13	106 (ν) Pisc	f	11	56	f	1	11	1	pB. S. iR. mbM.
271	}	—	f	14	54	n	0	11	3	{ Two, very close. nearly par.
272		—	f	14	54	n	0	11	3	{ The f. smallest and most n.
273	—	86 (γ) Ceti	p	0	14	n	1	44	1	F. S. iR.
274	—	92 (α) Ceti	p	3	54	f	0	47	1	F. vS. iE. er.
275	20	32 (2 τ) Hyd	f	9	55	n	1	32	2	pB. cL. iR.
276	—	10 (r) Virgin	p	6	58	n	0	5	3	F. pL. R. lbM.
277	—	—	p	5	14	f	0	1	3	F. S.
	1785									
278	Jan. 6	75 Ceti	p	1	38	f	0	5	1	pB. S. E.
279	—	35 Eridani	f	2	55	f	0	38	2	F. mE. vlbM. about 4' l.
280	—	14 Hydræ	f	5	2	n	0	21	1	F. vS. lE. ver. 240.
281	—	28 (A) Hydr	p	29	27	n	1	40	2	F. vS. E.
282	10	41 Ceti	f	17	28	n	0	20	2	p3. cL. lE. mbM.
283	—	—	f	21	26	n	0	10	2	pB. S. mbM.
284	—	80 Ceti	f	3	34	f	0	19	1	F. mE. about 3' l and ¾' b.
285	—	55 (ζ) Ceti	f	74	50	n	1	2	2	pB. E. sp nf. about 1½ l.
286	—	—	p	4	34	f	0	9	1	F. pL. R. lbM. f. Sft.

II.	1785	Stars.		M. S.	D.M.	Ob.	Description.
287	Jan. 27	17 Eridani	p	10 24	f	1 12 2	F. vS. IE. er. unequally B.
288	28	21 Eridani	p	1 55	n	0 35 3	F. pL. iR. r.
289	31	7 (v) Lepor	f	2 32	n	0 51 1	F. pL. i triangular F. r.
290	Feb. 1	89 (π) Ceti	f	49 17	n	0 21 3	F. pL. R. lbM. f. pLst.
291	—	26 (π) Erid	p	3 39	f	1 25 1	pF. mE. mer. 3 or 4' l and 1' b.
292	—	45 (μ) Lepor	p	0 50	n	0 29 1	pB. iR. mbM. ip. pcst.
293	—	76 (3b) Crater	p	52 51	n	0 23 1	pB. S. iR. bM.
294	—	31 Crateris	p	6 45	n	0 6 1	F. S. E. r.
295	—	—	p	1 48	n	1 18 1	F. vS. iF. bM.
296	—	—	p	0 12	n	0 24 1	pB. pL.
297	—	89 Virginis	p	11 47	n	0 18 1	pF. L. mbM.
298	—	88 (η) Corvi	f	18 44	n	1 51 1	{ F. pL. lbM. 1' p. is a S suf- pected stellar.
299	—	53 Virginis	p	12 30	n	0 48 1	pB. pL. mbM.
300	—	—	p	11 0	n	2 8 2	pF. cl.
301	—	—	p	3 8	n	0 34 1	pB. pL. iR. mbM.
302	—	2 (1α) Cancr	p	3 5	f	1 40 1	pF. vS. bM. er.
303	—	19 (λ) Cancr	p	2 22	f	0 35 1	F. S. mbM. r.
304	Mar. 4	11 Monoc	f	30 53	f	0 37 3	Some Sst with pB nebuloity.
305	—	20 Sextantis	p	7 14	n	0 49 1	F. S. IE. er.
306	—	88 Virginis	f	0 52	f	0 24 1	F. vS. iF. r.
307	—	—	f	3 58	n	0 43 1	F. cL. iF. bM.
308	—	82 (m) Virg	f	12 28	n	1 6 2	F. S. iR. lbM.
309 } 310 }	—	99 (ε) Virg	p	12 31	f	0 1 1	{ Two. nearly mer. dist. 4' Sst. betw. che. touch. { n. pB. cL. mbM. { f. F. S.
311	—	106 (3b) Crate	p	68 34	f	1 18 2	cB. S. mbM.
312	—	45 (γ) Hydr	f	9 41	n	2 0 1	F. L. iR. vgbM.
313	—	—	f	10 53	n	1 16 1	pB. IE. par. b towards f. side.
314	—	—	f	17 57	n	1 55 1	F. S. iF. bM.
315	—	11 23 (2φ) Can	f	0 29	f	1 0 2	F. S. R. bM. C. N.
316 } 317 }	—	12 64 (1b) Gem	p	4 16	n	1 17 1	{ Two. 1p nf. dist. 1' che. mix. Both F. S. equal. N.
318	—	22 (1φ) Can	f	8 38	n	0 36 1	F. pL. IE. mbM. r.
319	—	48 (1α) Canc	p	9 10	f	0 5 1	F. S. bM. r.
320	—	13 23 Leonis min	p	12 38	n	1 50 1	F. pS. R. lbM.
321	—	13 Can. ven.	p	51 31	f	0 50 1	pB. L. gbM.
322 } 323 }	—	—	p	40 19	f	1 28 1	{ The two first of 3 in a line. of unequal size and brightness.
324	—	—	p	38 3	n	0 17 1	F. S.
325	—	—	p	26 51	f	0 30 1	F. pL. E. bM.
326	—	—	p	14 11	f	0 4 1	F. mE. mer.
327	—	—	f	19 43	f	0 35 1	F. pS.
328	—	—	f	23 43	n	0 38 1	pB. pS. nearly R. mbM.

II.	1785	Stars.		M. S.		D.M.	Ob.	Description.
329	Mar. 13	49 (δ) Bootis	p	48 50	n	0 5 3		pF. S. R. r. n. 2 pBf.
330	—	—	p	45 45	f	2 2 1		pB. pL. R. bM.
331	16	11 Urfæ min	p	60 36	f	0 2 1		F. pS. er.
332	—	—	p	20 10	f	0 2 1		pB. cL. b towards p. side.
333	April 3	27 Urfæ	f	20 14	f	0 2 1		{ Two. Nearly mer. Most n. pB. pS. bM. Most f. F. S. bM.
334			f	73 0	n	1 41 1		
335	—	—	f	88 16	n	0 30 1		pB. vS. iR.
336	—	—	f	94 42	n	0 48 1		pF. pS. bM.
337	6	44 Leonis min	f	31 8	f	0 59 2		F. cL. iR. gvlbM.
338	—	53 Leonis min	f	19 26	n	1 0 1		pF. pS. iF.
339	—	72 Leonis	f	25 8	n	1 35 2		F. vS. stellar. short ray p. side.
340	—	4 Comæ	p	29 46	n	0 35 1		F. stellar.
341	—	—	p	22 2	n	0 37 1		F. pL.
342	—	21 (g) Comæ	f	4 36	n	1 56 1		not L.
343	—	—	f	20 56	n	1 11 1		F. pL. lE.
344	—	—	f	23 34	n	2 28 1		just f. pBf.
345	—	31 Comæ	f	4 54	f	0 34 2		F. pL. iF.
346	10	36 (ζ) Leonis	f	11 14	f	0 33 1		pB. S. bM. r.
347	—	41 Leonis min	p	3 34	n	0 51 1		F. S. lE.
348	—	72 Leonis	f	14 12	n	1 1 1		F. pL. i triangular F.
349	—	—	f	16 2	f	0 17 1		F. S.
350	—	—	f	18 2	n	1 22 1		F. S.
351	—	92 Leonis	p	3 9	n	1 16 2		F. pS.
352	—	7 (b) Comæ	p	4 37	n	0 11 1		pB. cL. iF. bM.
353	—	—	p	0 43	n	0 2 1		F. vS.
354	—	22 Comæ	p	4 32	f	1 28 1		pF. L. broadly E.
355	—	40 Comæ	f	5 38	n	1 25 1		pB. S.
356	—	12 (d) Bootis	f	18 46	f	1 53 1		F. S. iF. lbM.
357	11	39 Leonis mi	p	8 4	n	0 8 1		F. pL.
358	—	—	p	7 38	n	1 1 1		pB. pS. nearly R. bM.
359	—	44 Leonis mi	p	1 46	n	0 39 1		F. pL. iF.
360	—	—	f	0 50	n	0 30 1		F.
361	—	—	f	1 18	n	0 1 1		pB. pL.
362	—	—	f	1 32	n	0 8 1		F. S.
363	—	—	f	4 35	f	0 44 1		pF. pL. lE. b towards ff. side.
364	—	—	f	14 2	n	0 56 1		F. mE. $1\frac{1}{2}$ l. but v. narrow.
365	—	—	f	14 24	n	0 4 1		pF. pL.
366	—	—	f	42 12	n	0 14 1		F. vS.
367	—	14 (b) Comæ	p	28 42	n	0 59 1		pF. bM.
368	—	—	p	27 58	n	0 12 1		F. pL. E. b towards f. side.
369	—	—	p	20 16	n	0 55 1		pB. cL. mb towards nf. side.
370	—	—	p	17 40	n	1 55 1		One of three. F. iF.
371	—	—	p	—	—	—		

Third class. . Very faint nebulae.

III.	1784	Stars.		M. S.		D.M.	Ob.	Description.
50	Mar. 19	45 (1A) Can	f	3 15	f	0 4	1	eF. ver. 240. and cL. R.
51	}	27 (v) Leonis	p	7 0	n	0 21	1	{ Two. np. ff. 6 or 7' dist. Both eF. p is the largest.
52		34 Leonis	f	1 0	f	0 41	2	
53		52 (K) Leon	p	10 45	f	1 27	1	eF. S. lE. r. 3 or 4 ft in it.
54		46 (i) Leonis	f	4 18	n	0 3	2	eF. cL. R. r. no N.
55		15 Bootis	p	13 0	f	0 40	1	vF. vS. iR. r. some ft. in it.
56	—	—	p	10 30	f	0 28	1	eF. vS. E. r.
57	—	—	p	8 30	f	0 43	1	eF. S. ver 240.
58	—	—	p	6 15	f	1 10	1	eF. S. ver 240 and lE.
59	—	—	p	6 15	f	1 10	1	eF. S. ver 240.
60	21	47 (d) Cancr	f	20 0	n	0 23	1	vF. S. with 240 near Sft.
61	—	—	f	26 30	f	0 18	1	eF. 240 shewed 5 Sft with nebulos.
62	}	—	f	31 30	n	0 50	1	{ Two. nearly mer. Both vF. pS. R. lbM. r. with 240 cL.
63		—	f	36 0	n	0 52	1	
64		51 (m) Leon	p	38 15	f	0 33	1	eF. 240 shewed some Sft with neb.
65	—	—	p	9 15	f	0 44	1	vS. E. r. better with 240
66	—	—	p	11 45	f	1 45	1	vF. S. E. r. the same with 240
67	—	—	f	1 45	f	0 40	1	vF. nebul. betw. 2 ft. 2'l. ver. 240
68	—	3 Comæ	p	5 0	f	0 18	1	2 vSft with susp. neb. 240 doubtf.
69	—	25 Comæ	p	6 0	f	0 42	1	vF. S.
70	—	27 Comæ	f	19 30	f	0 41	1	vF. not S.
71	—	42 Comæ	f	10 15	f	1 26	1	{ 3 Sft with suspect. nebul. 240 left some doubt.
72	—	4 (r) Bootis	p	4 0	f	1 50	1	
73	—	5 (r) Herc	p	1 15	n	0 5	1	eF. vS. ver 240 and cL.
74	—	48 Serpensis	p	12 24	f	1 7	1	eF. vS. easily ver. 240.
75	Apr. 8	70 (h) Leonis	p	4 0	f	0 41	1	vF. S. ver. 240
76	—	—	f	4 0	f	0 41	1	eF. not S.
77	—	94 (b) Leonis	f	12 12	f	1 12	1	eF. pL. easily ver. 240.
78	—	6 Comæ	f	17 18	f	0 19	1	eF. pL. R. r.
79	12	73 (n) Leonis	p	5 6	f	1 25	1	vF. r. by moon-light.
80	—	—	f	18 36	f	0 48	1	eF. not L. lE. r.
81	—	—	f	22 35	f	1 11	2	vF. vS. R. bM. stellar. ver. 240
82	—	41 Virginis	p	1 42	n	1 7	1	vF. vS. R. stellar.
83	—	—	f	6 18	n	0 0	1	vF. S. E. r.
84	—	70 Virginis	p	3 42	f	0 4	1	vF. S. iF. r.
85	}	—	f	6 12	n	0 1	1	eF. vS. stellar. ver. 240.
86		—	f	6 48	f	0 9	1	{ Three. The two p. vF. S. R. The last vF. pL. R. Place of the 2d not taken.
87		—	f	5 42	f	0 23	1	
88	13	56 Leonis	p	6 24	f	1 29	1	eF. no time to ver.
89	—	63 (x) Leon	f	4 54	n	0 1	1	eF. a little doubtful.
90	—	3 (v) Virginis	f	7 48	n	1 19	1	vF. vS. vlbM.
91	—	11 (s) Virg	f					The f. of 2. eF. II. 17.

III.	1784	Stars.		M. S.		D.M.	Ob.	Description.
92	April 13	9 (o) Virginis	f	16 15	f	2 4	2	{ Two. One vF. vS. The other just by. eF. eS. left doubtful.
93								
94								
95								
96								
97	—	31 (1 d) Virg	p	17 9	n	0 42	2	{ The smallest of 2. eF. II. 144. eF. eS. The place not accurate.
98	—	—	p	3 6	n	0 0	1	
99	—	32 (2 d) Virg	f	47 36	f	0 33	1	eF. S.
100	—	—	f	50 42	f	1 8	1	eF. E.
101	—	—	f	51 0	f	0 23	1	eF. pL. R. er. The st almost visible
102	15	2 (1 d) Virgin	p	1 48	n	1 54	1	eF. pL.
103	—	—	f	0 24	n	0 58	1	vF. r.
104	—	4 (2 d) Virgin	p	2 12	n	0 19	1	vF. vS. left doubtful. Twilight.
105	—	31 (1 d) Virg	p	1 52	n	1 35	2	eF. vL. lbM.
106	—	33 Virginis	f	7 30	n	0 8	1	vF. pL. vlbM. r.
107	17	48 Leonis	f	6 54	0	8 1	1	eF. pL. a little doubtful. Twil.
108	—	63 x) Leonis	p	13 18	n	1 7	1	eF. eS. r.
109	—	90 Leonis	f	5 18	n	0 49	1	{ 8 or 10' sp. II. 161. vS. stellar. not ver.
110	—	20 Bootis	f	1 54	f	2 29	1	vF. vS. lE. ver. 240.
111	18	{ 58 (d) Leon 84 (r)	f	8 36	1	{ vF. vS. r. ver. 240.
112	24	74 (p) Leonis	f	10 6	f	1 52	1	eF. cL. R. r. near vBft. D light.
113	—	—	f	34 18	f	1 3	1	eF. eS. with 240. 2 vSft and nebu.
114	25	28 Virginis	p	14 18	n	1 35	1	{ 2 vSft with nebulousity with 240 left doubtful.
115	May 9	67 (a) Virg	f	1 12	f	1 10	1	vS. vF. stellar. ver. 240.
116		31 (t) Libræ	p	8 48	n	0 15	1	vF. cL. nearly R. lm.
117	{	11 100 (λ) Virg	p	59 30	n	0 48	1	{ The two most f. of 3. That M. vF. vS. The most f. eF. eS. ver. 240. II. 193.
118								
119	—	—	p	55 42	n	0 29	1	eF. vS. stellar. ver. 240.
120	—	—	f	6 24	n	0 9	1	eF. pL. iR. lb towards f. side.
121	{	14 9 (a) Libræ	p	27 0	f	0 36	1	{ Two np ff. The f. eF. 1' d. nearly R. The p. vF. vS. R. dist. 5'.
122								
123	—	15 18 Herculis	f	40 30	f	0 47	1	vF. pL. R. lbM.
124	—	—	f	43 30	f	0 47	1	vF. stellar. ver. 240.
125	16	25 (p) Bootis	p	33 12	f	1 10	1	vF. S. iR. lbM. almost stellar.
126	—	—	p	2 18	f	0 24	1	{ 2 Sft. with suspected nebul. al- most ver. 240.
127	{	— 28 (σ) Bootis	f	3 48	n	0 45	1	{ Two. 3' dist. par. The f. vF. vS. iR. The p. eF. vS. ver. 240.
128								

III.	1784	Stars.		M. S.		D.M.	Ob.	Description.
171	Sept. 13	43 (β) Andr	f	17 30	f	0 56	1	stellar.
172		— — —	f	18 0	f	2 8	1	{ Two. Both vS. stellar. a little doubtful.
173		— — —	f	18 0	f	2 8	1	
174		3 (α) Triang	p	25 24	n	0 22	1	stellar. ver. 240.
175		— — —	p	12 48	n	2 29	1	stellar.
176		— — —	p	6 6	n	1 0	1	eF. stellar. 240 left some doubt.
177		9 (γ) Triang	f	9 36	f	0 17	1	vF. cL. iR. r. 2 or 3' d.
178		17 (γ) Persei	f	9 6	n	0 13	1	vF. pL. R. SB place M.
179		15 6 (β) Arietis	p	3 0	n	1 30	1	vF. pL. 1E.
180		18 40 Pegasi	p	3 0	n	0 47	1	eF. vS. R. n. cLst.
181		65 Pegasi	p	6 48	f	1 49	1	vF. vS. R. ver. 240.
182		40 Pegasi	f	38 24	f	0 51	2	4 or 5 Sft. with nebul. 240 doubt.
183		89 (χ) Pegasi	f	0 30	f	1 38	1	eF. S. iE.
184		20 11 Piscium	p	17 44	f	0 22	2	eF. vS. stellar. ver. 240.
185		— — —	p	12 50	f	0 32	2	vF. E. er. 3 fSt. visible in it.
186		20 Piscium	p	29 15	f	1 41	1	eF. vS.
187		— — —	p	14 39	n	0 1	1	eF. stellar. ver. 240 and cL.
188		— — —	p	13 33	f	0 9	1	eF. stellar. just like 187.
189		— — —	p	8 15	f	1 52	1	eF.
190		29 Piscium	f	4 54	f	0 40	1	vF. vS.
191		34 Ceti	p	9 12	f	1 53	2	vF. mE.
192		72 Ceti	p	17 24	f	1 43	1	eF. S. ver. 240. with difficulty.
193		— — —	p	12 12	f	2 6	1	eF. ver. 240. with difficulty.
194		81 Ceti	f	38 6	n	0 55	1	eF. eS.
195		— — —	f	42 42	n	0 49	1	eF. eS. ver. 240.
196		— — —	f	47 0	n	0 36	1	{ Two. Both eF. ver. 240 but just suspected with 157.
197		— — —	f	47 0	n	0 36	1	
198	Oct. 6	12 (q) Persei	p	3 3	n	0 40	2	cB. mE. vgmbM. near 4' l.
199		7 27 (α) Persei	p	8 27	n	0 2	2	The f. of 2. vF. iF. pS. II. 239.
200		14 53 Piscium	f	4 24	n	1 13	2	2 Sft with nebulosity ver. 240.
201		19 Arietis	f	4 6	f	0 47	1	vF. vS. E. f. pCst.
202		15 47 Piscium	p	83 54	f	1 15	1	eF. vS. stellar. ver. 240.
203		— — —	p	78 18	n	0 18	1	vF. cL. E. 2' l.
204		59 Piscium	f	0 42	n	0 2	1	vF. S. sp. 2 vSft.
205		92 Piscium	p	5 30	f	0 10	1	eF. ver. 240. discovered in gaging.
206		— — —	p	3 30	f	1 20	1	eF. S.
207		8 (α) Arietis	f	5 12	n	0 32	1	eF. vS. stellar. plainly. ver. 240.
208		— — —	f	6 30	f	1 49	1	eF. vS. iR. just f. pBft.
209		16 17 Delphini	f	18 6	f	0 11	1	vF. S. R.
210		— — —	p	2 48	n	0 46	1	{ Two. The p. vF. S. 1E. The f. vF. vS. stellar.
211		54 (α) Pegasi	p	2 48	n	0 46	1	
212		— — —	f	21 6	f	0 59	1	eF. eS. ver. 240. completely though with difficulty.
213		— — —	f	27 36	n	0 40	1	eF. cL. ver. 240. betw. 2 Bft.

III.	1784	Stars.		M. S.	D.M.	Ob	Description.
214	Oct. 16	31 Arietis	p	36 48 n	1 24 1	1	vF. stellar. ver. 240.
215	—	—	p	36 6 n	0 6 1	1	eF. stellar. discovered by 240.
216	18	46 (ξ) Pegasi	f	3 15 f	0 37	3	{ Two. The p. vF. pS R. vlbM. The f. vF. pS. R. vlbM.
217			f	3 25 f	0 32		
218			f	13 51 n	0 4 1		
219	19	15 Delphini	p	5 24 n	0 2 1	1	eF. vS. stellar. ver. 240. with dif.
220	—	66 Pegasi	p	10 10 n	0 23 4	4	F. R. bM. 1' $\frac{1}{2}$ d.
221	—	—	p	7 10 n	1 0 2	2	vF. S.
222	—	—	p	7 7 n	0 54 2	2	vF. S. R.
223	20	7 (<i>b</i>) Ceti	f	23 12 f	1 1 1	1	vF. lE. or oval. 1' d. np. 2 pBst.
224	—	1 (1 τ) Erid	p	21 42 f	2 11 2	2	vF. S. iR.
225	—	15 (δ) Lepor	f	6 24 n	0 49 1	1	eF. E. r. near 1' l. ver. 240.
226	21	70 (<i>g</i>) Pegasi	p	1 50 f	0 18 2	2	vF. vS. stellar ver. 240.
227	Nov. 7	64 Ceti	p	2 24 f	0 37 1	1	2 or 3 Sst. with neb. nearly ver. 240
228	—	73 (2 ξ) Ceti	f	12 54 n	0 17 1	1	{ Two about 1' dist. The p. eF. vS. ver. 240. The f. eF. eS. 240. doubtf.
229							
230	12	55 (<i>l</i>) Pegasi	p	3 36 f	0 29 1	1	eF. eS. 240 left some doubt.
231	—	31 (1 ϵ) Pisc	p	9 0 f	1 0 1	1	Two. Both vF. stellar.
232							
233	—	—	p	8 27 f	1 0 2	2	eF. pL. glbM.
234	16	43 (γ) Canc	p	11 24 n	1 6 1	1	vF. stellar.
235	—	—	p	3 20 n	2 4 1	1	eF. S. ver. 240.
236	—	4 (λ) Leonis	p	23 22 f	1 37 1	1	eF. lE. betw. 2 pBst. ver. 240.
237	17	33 Pegasi	f	12 54 n	0 46 1	1	eF. vS.
238	—	66 Pegasi	p	6 6 n	1 10 1	1	eF. eS. ver. 240. with difficulty.
239	—	4 Eridani	p	32 26 f	1 1 1	1	vF. S. 1' dia. or more.
240	20	12 Leporis	p	7 55 f	0 59 1	1	vF. vS. stellar.
241	—	—	f	3 39 n	0 23 1	1	eF. vS. lE. par.
242	—	15 (ι) Nav	f	68 16 n	0 53 1	1	vF. lE. S. 1' d.
243	Dec. 2	56 Pegasi	p	9 16 n	0 42 1	1	vF. S. er.
244	9	48 Ceti	p	48 34 n	0 27 1	1	eF. vS. E.
245	—	15 Eridani	p	15 49 f	0 27 1	1	vF. cL. iE. r. unequally B.
246	—	19 Eridani	p	1 38 n	0 50 2	2	vF. E. equally B.
247	—	—	f	6 5 f	1 4 1	1	eF. vS.
248	—	27 Eridani	p	4 23 n	1 7 1	1	vF. vS. lE.
249	—	—	p	2 19 n	1 18 1	1	vF. vS.
250	13	89 (<i>f</i>) Pisc	f	2 25 f	0 14 1	1	{ Two. nearly par. 4 or 5' dist. Both vF. vS. R.
251							
252	—	—	f	3 42 n	1 38 1	1	vF. pL. iR. lbM.
253	—	—	f	6 48 n	0 11 1	1	eF. cL. E.
254	—	15 Sextantis	p	14 34 n	1 52 2	2	vF. E. np ff. 5' l. $\frac{1}{2}$ b.
255	—	7 Sextantis	f	20 27 n	0 42 1	1	vF. vS. p. triangle of Bst.
256	20	13 (ζ) Can. mil	f	26 5 f	0 48 1	1	vF. vS. ver. 240.

III.	1784	Stars.		M. S	D.M.	Ob.	Description.
257	Dec. 20	13 (γ) Can. mi	f	44 59	f	0 55 1	eF. pL. iF.
258	—	10 (·) Virgin	p	5 2	f	0 7 2	vF. S. E.
259	1785						
260	Jan. 6	70 Ceti	p	10 34	f	0 38 1	eF. eS. iF.
261	—	—	p	7 10	n	0 4 1	eF. vS. stellar.
262	—	75 Ceti	p	3 46	f	0 6 1	vF. cL.
263	—	94 Ceti	p	1 16	f	1 15 1	eF. ver. 240 with difficulty.
264	—	24 Eridani	p	3 22	f	0 11 1	eF. stellar. or IE. almost ver. 240.
265	—	28 (A) Hydr	p	26 48	n	1 19 2	vF. vS. R. ver. 240.
266	—	45 (θ) Ceti	f	32 28	f	0 46 1	eF. stellar. ver. 240.
267	—	—	f	31 6	f	0 43 1	vF. IE, ver. 240.
268	Feb. 4	14 (γ) Lepor	f	0 1	f	1 56 1	vF. pS. iE. bM.
269	—	11 (α) Lepor	p	27 51	f	0 31 1	eF. vS. stellar. ver. 240. easily.
270	—	19 Leporis	p	32 23	n	1 11 1	eF. vS. stellar. ver. 240. easily.
271	—	—	p	20 0	n	1 28 1	vF. eS. stellar. ver 240 difficulty.
272	—	8 (3 γ) Can	f	8 0	n	0 4 1	3 or 4 Sft with neb. vF. ver. 240.
273	—	76 (3 b) Crater	p	58 39	n	1 21 1	vF. pS. iF. vlbM.
274	—	—	p	55 43	n	0 39 1	eF. vS. iF.
275	—	31 Crateris	p	4 40	f	0 14 1	vF. pL. iF.
276	—	8 12 Hydræ	f	20 30	f	1 49 1	vF. vS. bM. ½ f. Sft.
277	—	38 (α) Hyd	p	9 20	f	0 26 1	vF. vS. stellar. 240. the same.
278	—	39 (1 v) Hyd	p	5 0	n	0 30 1	{ Two. 3 or 4' dist. The most n. vF. S. The f. vF. vS. Both stell.
279	—	8 (α) Corvi	p	31 26	n	0 16 1	eF. pL. better with 157 than 240.
280	—	—	f	18 44	n	1 51 1	{ ½ p. II. 298. eF. eS. stell. 240. doubtful.
281	—	—	f	20 38	n	0 46 1	vF. pS. r.
282	—	53 Virginis	f	7 12	n	1 12 1	vF. mE. ff np. v narrow.
283	17	41 (α) Bootis	p	27 54	n	0 27 1	vF. vS.
284	Mar. 5	25 (f) Virg	p	54 12	f	0 19 1	vF. S. iE. lbM.
285	—	88 Virginis	f	8 45	n	1 17 1	eF. vS.
286	—	99 (α) Virg	p	9 22	n	0 31 1	vF. L. b towards n.
287	—	—	p	7 58	f	0 7 1	vF. pS. iF.
288	—	6 15 (α) Navis	f	11 16	f	1 7 1	vF. cL. er. some of the ft. vis.
289	—	10 6 (3 b) Crat	p	69 14	f	0 25 2	F. vS. large stellar. lbM.
290	—	2 (α) Corvi	p	16 1	n	2 3 1	eF. pL. broadly E. nearly par.
291	—	11 75 Cancr	p	2 53	f	1 13 1	vF. pL. R. bM.
292	—	12 46 Cancr	p	11 46	f	1 14 2	vF. pL. R. lbM. r.
293	—	23 Leonis	p	17 46	f	0 22 1	eF. eS. ver. 240.
294	—	13 57 (2 α) Canc	p	2 44	n	0 15 1	vF. vS. R. bM. large stellar.
295	—	72 (α) Canc	f	5 47	n	0 24 1	vF. vS. R. nf. 2pBft.
296	—	—	f	8 42	n	1 17 1	vF. S. R. lbM.
297	—	15 (f) Leon	p	13 8	f	0 34 1	eF. eS. 240 left a doubt.
298	—	18 Leonis min	p	20 56	f	0 44 2	vF. vS. iR. lbM.

IV.	1784	Stars.		M. S.	D.M.	Ob.	Description.
18	Oct. 6	14 Androm.	p	6 11 n	3 16	4	B. R. a planetary p. well defined disk. 15" dia ^r with a 7 feet reflector.
19	16	5 Monoc.	p	7 6 f	0 10	1	A st. of the 9 magnitude, with m. chev. 1 elliptical.
20	—	—	p	3 42 n	0 3	1	A st. of the 11 or 12 mag. affected like the foregoing, but vF.
21	Nov. 20	12 Leporis	p	8 48 n	0 24	1	vS. stellar. vBN. and vF. chev. not quite central.
22	Dec. 9	7 (ξ) Navis	f	3 10 f	1 28	2	L. pB. R. er. 6 or 7' d. a faint red colour visible. A st. 8 mag. not far from the center, but not connected. 2d ob. 9 or 10' d.
23	1785 Jan. 6	75 Ceti	p	4 40 f	0 6	1	cB. a vBN. with a chev. of 3 or 4' d.
24	—	50 (ζ) Orio	f	0 57 f	0 17	1	A Bst. with m. chev. 5' l. 4' b.
25	31	19 Navis	p	67 0 n	1 15	1	A pcst. with vF. and vS. m. chev. iF.
26	Feb. 1	34 (γ) Erid	f	16 16 n	0 49	2	vB. perfectly R. or vl. elliptical. planetary but ill defined disk. 2d obs. r. on the borders, and is probably a very compressed cluster of stars at an immense distance.
27	—	76 (3b) Crater	p	28 39 n	1 25	2	Beautiful, brilliant, planetary disk ill defined, but uniformly B. the light of the colour of Jupiter. 40" d. 2d obs. near 1' d. by estimation.
28	—	31 Crateris	f	1 0 n	0 47	1	pB. L. opening with a branch, or two nebulae very faintly joined. The f. is smallest.
29	—	84 (v) Crateris	f	3 36 n	0 16	1	A St. with an eF. brush p. perceived in gaging. ver. 240.

Fifth class. Very large nebulae.

V.	1783	Stars.		M. S.	D.M.	Ob.	Description.
1	Oct. 30	18 (s) Pis. auct.	f	128 17 n	1 39	6	cB. mE. sp nf. mbM. Above 50' l. and 7 or 8' b. C. H. See note.
2	1784 Jan. 24	10 (r) Virgin	f	24 46 n	0 17	4	cB. mE. np ff. mbM. er. 9 or 10' l with a branch towards the ap.

S f f 2

V.

Sixth class. Very compressed and rich clusters of stars.

Additional
abbreviations

} Cl. Cluster.
sc. scattered.

com. compressed.
co. coarsely.

Eighth class. Coarsely scattered clusters of stars.

VIII.	1783	Stars.		M. S.	D.M.	Ob.	Description.
1	Dec. 3	14 Navis	p	4 0 n	0 40	2	A Cl. of co. sc. st. The place is that of the most com. part which is not M.
2	26	58 (α) Orion	p	8 28 n	1 16	2	A S. Cl. of vs. sc. st.
3	—	13 Monocer	f	1 30 n	1 2	2	An E. Cl. of L. sc. st.

VIII.

VIII	1784	Stars.		M. S.	D.M.	Ob.	Description.
4	Jan. 16	112(β) Tauri	p	0 51	0 38	3	A Cl. of co. and i. sc. pLft.
5	18	15 Monocer	p	0 0	0 0	3	Double and attended by more than 30 cLft.
6	24	8 Monocer	p	14 20	0 4	2	A Cl. of co. sc. ft. not rich.
7	Feb. 10	4 Orionis	p	4 0	1 7	1	A Cl. of L. and S. sc. ft. not rich.
8	15	97 (ι) Tauri	p	5 28	0 13	2	A Cl. of cL. v. co. sc. ft. perhaps a projecting point of the m way.
9	19	24 (γ) Gemi	p	8 15	0 15	1	A Cl. of vin. sc. ft. of various magnit. near $\frac{1}{2}$ deg. not rich.
10	Mar. 15	50(2A) Cane	f	3 0	0 44	1	A Cl. of v. co. sc. ft. not rich.
11	16	50 Gemini	f	15 55	2 19	1	A Cl. of sc. ft.
12	June 16	1 (m) Aquilæ	f	1 42	0 2	1	A Cl. of v. co. sc. ft.
13	—	20 Aquilæ	p	12 48	0 56	1	A Cl. of co. sc. ft. not rich.
14	18	43 (δ) Sagitt	p	44 48	1 54	1	A Cl. of sc. pLft.
15	July 15	63 Sagittarii	p	103 36	2 1	1	A Cl. of co. sc. ft.
16	17	12 (ϕ) Cygni	f	13 6	0 44	1	A Cl. of not v. com. ft. closest M. It may be called (if the expression be allowed) a forming Cl. or one that seems to be gathering
17	18	33 Vulpec	p	24 18	0 4	1	A Cl. of many L. sc. ft.
18	Sept. 4	61 (ϕ) Aquilæ	p	2 54	0 18	1	A S. forming Cl. of ft.
19	—	—	p	0 42	0 40	1	A Cl. of co. sc. L. ft. not rich.
20	9	18 Vulpec	f	1 0	0 27	1	A Cl. of co. sc. ft. not rich.
21	10	6 Vulpec	p	2 27	0 29	2	A Cl. of cL. co. sc. ft.
22	—	18 Vulpec	f	1 12	0 12	1	A Cl. of co. sc. ft.
23	Oct. 15	12 (γ) Delph	p	5 18	0 33	1	A Cl. of co. sc. ft.
24	—	67 (ν) Orion	f	1 0	0 46	1	A SCl. of pL. white ft.
25	16	10 Monocer	f	0 0	0 0	1	The 10 Monoc. surrounded by by many Bft.
26	Nov. 16	1 (H) Gem	p	2 16	0 3	1	A Cl. of ft. of various magnit. not v. rich. 6 or 7' d.
27	20	11 (ϵ) Navis	p	36 41	0 46	1	A S. Cl. of sc. ft. not rich, nor v. com.
28	Dec. 5	54 (1 λ) Orion	p	11 53	0 15	1	A Cl. of pL. sc. ft. not rich.
29	9	101 (4 b) Aqu	f	32 30	0 11	1	A Cl. of a few co. sc. L. ft.
30	—	25 (δ) Canis	f	57 10	1 15	1	A vL. Cl. of many co. sc. L. ft.
31	1785 Jan. 6	19 Monocer	p	15 36	1 3	1	A L. Cl. of sc. ft. not v. rich.
32	10	26 Monocer	p	34 32	0 41	1	A Cl. of co. sc. ft. of many magn. p. rich. above 15' d.
33	—	—	p	32 50	1 15	1	A Cl. of sc. L. ft.
34	—	—	p	26 36	0 52	1	An extensive Cl. of sc. ft.
35	31	2 Navis	p	21 23	1 21	3	A Cl. of pL. sc. ft. p. rich. about 20' l. crooked fig.

Notes to some Nebulæ and Clusters of Stars.

I. 7. This remarkable appearance being no longer in the place it has been observed, we must look upon it as a very considerable telescopic comet. It was visible in the finder and resembled one of the bright nebulæ of the *Connoissance des Temps* so much, that I took it for one of them till I came to settle its place; but this not being done till a month or two after the observation, the opportunity of pursuing and investigating its track was lost.

I. 13.. The figures referred to, in the description of this and some other nebulæ, may be found in the *Philosophical Transactions*, vol. LXXIV. tab. XVII. p. 450.

I. 28. The numbers annexed to some of the nebulæ refer to the class and number of the preceding Catalogue: thus, II. 41. denotes that the 41st in the second class is the third nebula, following the two here described.

I. 28. Near the 84. and 86. neb. of the *Connoissance des Temps*.

II. 6. This has probably been a telescopic comet, as I have not been able to find it again, notwithstanding the assistance of a drawing which represents the telescopic stars in its neighbourhood.

II. 55. The preceding is the 85 of the *Connoissance des Temps*.

II. 84 6 or 8' following the 100 of the *Connoissance des Temps*.

II. 118. Just following the 88. of the *Connoissance des Temps*.

II. 123. 124. The third is the 87th of the *Connoissance des Temps*.

III. 44. The following is the 60th of the *Connoissance des Temps*.

IV. 13. Before the value of the degree was more strictly ascertained, the two observations were thus:

21 Velpeculæ	f	2' 6"	n	1° 51'
39 (b) Cygni	p	8 6	f	1 35

which, if there be no error in the place of the stars in FLAMSTEED's Catalogue, differ about 14' in polar distance, for which reason in the second Paper on the Construction of the Heavens this nebula was put down twice, whereas it now appears, that both observations belong to the same.

V. 1. This nebula was discovered Sept. 23, 1783, by my sister CAROLINE HERSCHEL, with an excellent small Newtonian *Sweeper* of 27 inches focal length, and a power of 30. I have therefore marked it with the initial letters, C. H. of her name. See also V. 19. discovered Aug. 27, 1783, and VII. 13. discovered Feb. 26, 1783.

V. The *Front-view* is a method of using the reflecting telescope different from the Newtonian, Gregorian, and Cassagrain forms. It consists in looking with the eye glass, placed a little out of the axis, directly in at the front, without the interposition of a small speculum; and has the capital advantage of giving us almost double the light of the former constructions. In the year 1776 I tried it for the first time with a 10 feet reflector, and in 1784 again with a 20 feet one; but the success not immediately answering my expectations, it was too hastily laid aside. By a more careful repetition of the same experiment I find now, that several other considerable advantages, added to the brilliant light before mentioned, make it so valuable a construction that a judicious observer may avail himself of it at least in all cases where light is more particularly wanted; and from the experience of 30 sweeps, which I have already made with it, I may venture to announce it to be a very convenient and pleasant, as well as useful, way of observing. With regard to the position of objects, it differs from other constructions, by inverting the north and south, but not the preceding and following.

Errata of the Catalogue.

The following nebulæ should stand thus.

I. 54.	35 (r) Andr.	f	18 36	f	1 26	1	pB. S. R. vgbM.
II. 1.	41 Aquarii	p	11 45	n	0 17	2	F. cL. mE. bM. cr.
II. 239.	In the description read						The 2d of two.



XXVIII. *Investigation of the Cause of that Indistinctness of Vision which has been ascribed to the smallness of the Optic Pencil.* By William Herschel, LL.D. F.R.S.

Read June 22, 1786.

SOON after my first essays of using high powers with the Newtonian telescope, I began to doubt whether an opinion which has been entertained by several eminent authors, “ that vision will grow indistinct, when the optic pencils are less than the 40th or 50th part of an inch,” would hold good in all cases. To judge according to so rigid a criterion, I perceived that I was not intitled to see distinctly with a power much more than about 320, in a 7-foot telescope which bore an aperture of 6,4 inches; whereas in many experiments on double stars I found myself very well pleased with magnifiers that far exceeded such narrow limits. This induced me, as it were, by way of apology to myself, for seeing well where I ought to have seen less distinctly, to make a few experiments on the subject of the diameter of optic pencils. It occurred to me, that an opinion which limits them to any given size cannot be supported by theory, which does not determine on subjects of this nature, but must be decided, like many other physical questions relating to matters of fact, by careful experiments made upon the subject. The way, therefore, to come at truth, in a case which seemed to me of considerable importance, lay still open to me, as it had done to former observers;

servers; and I thought myself authorised, according to a Cartesian maxim (*Dubia etiam pro falsis habenda*), to suppose, for a while, the size of optic pencils, requisite for distinct vision, intirely undecided.

The first opportunity I had of making the proposed experiments was in the year 1778, and the result of them proved so decisive that I have never since resumed the subject; and had it not been for a late conversation with some of my highly esteemed and learned friends, I might probably have left the papers, on which these experiments were recorded, among the rest of those that are laid aside when they have afforded me the information I want. But a doubt seeming still to be entertained on the subject of the smallness of the optic pencils, it may now be proper for me to communicate these experiments, that it may appear how far the conclusions I have drawn from them are warranted by the facts on which I suppose them to rest.

Experiments with the naked eye.

Exp. 1. Through a very thin plate of brass I made a minute hole with the fine point of a needle; its magnified diameter, very accurately measured under a double microscope, I found to be ,465 of an inch, while under the same apparatus a line of ,05 in length gave a magnified image of 3,545 inches. Hence I concluded, that the real diameter of the perforation was about the 152d part of an inch. Through this small opening, held close to the eye, I could very distinctly read any printed letters on which I made the trial. Proper allowance must be made for the very inconvenient situation of the eye, which by the unusual closeness to the paper cannot be expected

to see with its common facility. Besides, the continual motion of the letters, which is required on account of the smallness of the field of view, must needs take up a considerable time.

Exp. 2. In some other pieces of brass I made smaller holes; and among many, that were measured with the same accuracy as in the former experiment, I found one whose magnified diameter was $.29$: hence the real diameter could not exceed the 244^{th} part of an inch. Through this opening I could also read the same letters; but the difficulty of managing so as not to intercept all the incident light, as well as the very uneasy situation of the eye, were sufficient reasons for not carrying the intended experiments any further under this form. Besides, I should hardly have allowed them to be fair, if, on a further contraction of the hole in the brass plate, an indistinctness had come on; as we might well have suspected at least two other causes, besides the smallness of the pencils, to contribute to such an imperfection; *viz.* want of light, and a deflection of it on the contracted edges of the hole.

Microscopic Experiments.

Exp. 3. I had now recourse to a double microscope, consisting, for simplicity's sake, of only two lenses. The focal length of the eye-glass, carefully ascertained by an object half a mile off, being $.9$; the distance of the object-glass from the eye-glass 9.36 ; and the aperture of the object-glass $.0405$. Hence we compute that the diameter of the optic pencil, when it entered the eye, could not exceed the 232^{d} part of an inch; yet with this construction I saw very distinctly every object I placed under the microscope.

Exp.

Exp. 4. I reduced the aperture of the object-glass to ,013; hence the pencil was found to be the 724th part of an inch; and yet I saw with this construction very distinctly every object that was placed under the magnifier.

Exp. 5. I made a second reduction of the aperture of the object-glass, so that now it was no more than ,0052; and therefore the optic pencil less than the 1800th part of an inch; and yet I could very well count the bristles on the edge of the wing of a fly, and distinguish their length and thickness.

Exp. 6. Changing the construction of the microscope, I now reduced the pencils by an increase of power. Solar focus of the eye-glass ,52; distance between the object-glass and eye-glass 7,6; aperture the same as in the third experiment. This gave me a pencil of the 336th part of an inch, with which I saw very distinctly.

Exp. 7. Applying now the reduced aperture of the fourth experiment, I had a pencil of the 1139th part of an inch, with which I saw very well.

Exp. 8. I changed the eye lens for another of ,171 focal length; the object-glass and distance between the two lenses remaining as in the two last experiments; aperture ,02. This gave a pencil of the 2173d part of an inch, with which I could count, or rather successively see, the bristles before mentioned very well; the field, on account of the great power, not taking in more than two large and a small one at a time.

Exp. 9. I was now convinced, that we may see distinctly with pencils incomparably less than the 40th or 50th part of an inch; and indeed so far from expecting any obstruction to distinct vision from the smallness of the pencils, it appeared to me now as if their size might in future be entirely left out of the account. With a view, however, of seeing what other causes

cause might bring on that indistinctness which had been ascribed to the smallness of the optic pencils, I continued these experiments with a variation in the apparatus, and used now an object lens of a different focal length; the aperture and other particulars being as in the 4th experiment. By this construction, which gave me a pencil of the 724th part of an inch, I could see objects very well; but though they appeared distinctly, they were not so sharp on the edges as one would wish to see them. This being compared with the 4th experiment, it appeared that, with equal pencils, unequal degrees of distinctness may take place; and a pretty striking circumstance, which served to lead me in the following experiments, was, that the smallest power gave me the least distinct image; notwithstanding, from former trials, the goodness of the lenses I employed could not be doubted.

Exp. 10. On an examination of circumstances it occurred to me, as indeed I had already before surmised, that a certain proportion of aperture might be necessary to a given focal length of an object-lens or speculum; and that a failure in this point might probably bring on that indistinctness which had been ascribed to the smallness of the pencils. In order, therefore, to put this to a trial, I used now an object-lens of 1,25 focal length, with an aperture confined to ,01; the rest of the apparatus being as in the 3d, 4th, and 5th experiments. The pencil in this case was about the 1000th part of an inch; and though by a different construction I had already seen very well with a pencil of not half that diameter, I found this to give me, as now I had reason to expect, a very indistinct picture, so much so indeed, that it could hardly be called a representation of the object.

Exp.

Exp. 11. Increasing the aperture of the object-lens to ,0124, I had a pencil of the 758th part of an inch, but could see no better with it.

Exp. 12. Proceeding in the track now pointed out to me, I admitted an aperture of ,017, which gave a pencil of the 550th part of an inch, but could see not much better with it than before.

Exp. 13. On a farther increase of the aperture to ,0231, and a pencil of the 406th part of an inch, I saw a little better; but still had not distinctness enough even to see the bristles before-mentioned at all. Hence we may conclude, that, in such constructions as the present one, the aperture of the object-glass must bear a considerable proportion to its focal length; since the 54th part (for ,0231 : 1,25 :: 1 : 54) is here not nearly sufficient.

Exp. 14. To the same apparatus I applied a higher power, by an exchange of the eye-glass; but the indistinctness remained as before.

Exp. 15. Returning again to the former construction, I admitted an aperture of about ,037; and having now a pencil of nearly the 250th part of an inch, I could but just perceive some of the large bristles, which shews that even the 34th part (for ,037 : 1,25 :: 1 : 34) of the focal length is not a sufficient aperture for object-lenses that act under such circumstances as the present.

So far I have only related experiments that were made in the year 1778; and my opinion that the smallness of the optic pencils could be no objection to seeing well being thus supported by evident facts, I hesitated not, in a Paper on the Parallax of the Fixed Stars (*Phil. Trans.* vol. LXXII. p. 96.) to affirm, that we might see distinctly with pencils much smaller

smaller than the 40th or 50th part of an inch. It did not appear to be necessary, nor would the subject of that Paper permit me to enter into a detail of experiments; but having, in the course of my reading about that time, met with an account of some very small globules made for microscopic uses, I contented myself with an instance of small pencils taken from them. I shall, however, now proceed just to hint at a few inferences that may be drawn from these related experiments; as, upon a mature consideration, we may find reason to believe they point out a cause of indistinctness of vision hitherto never noticed by optical writers; and which, when properly investigated, cannot but influence, and in some respects contribute to the improvement of, our theories in optics. For, admitting that every object-glass or speculum, whose aperture bears less than a certain ratio to its focal length, will begin to give an indistinct picture, it will follow, that while former opticians have been endeavouring to diminish the aberrations arising from the spherical figure, and the different refrangibility of rays, by increasing the focal length, they have been unaware of exposing themselves to the consequences of the cause of indistinctness here pointed out. And till its influence shall be well ascertained and brought to a proper theory, we must suspect that such tables as those which are given in our best authors of optics, pointing out an aperture of less than 6 inches for a glass of 120 feet focal length (or a ratio of 1 to 240) must be far from having that degree of perfection which may yet be obtained. No wonder that telescopes, made according to theories or tables, where one of the causes of indistinctness is unsuspected, and therefore left out of the account, can bear no smaller pencil than the 40th or 50th part of an inch! If then, on one hand, by increasing our apertures we

I

certainly

certainly run into great imperfections, we ought nevertheless also to consider what dangers, on the other, we may incur by lessening them too much.

As soon as convenient, I intend experimentally to pursue this subject, in order to obtain proper *data* for submitting this cause of optical imperfection to theory; at present my engagement with the work of a 40-feet reflector will hardly permit so much leisure; and till I shall have repeated, extended, and varied these experimental investigations, I would wish them to be looked upon as mere hints that may afford matter for future disquisitions to the theoretical optician.

P R E S E N T S

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ences at Paris.

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- Explication de la Planche qui représente plusieurs variétés de la Pierre aux Etoiles mouvantes. Hambourg, 1786. 4° Mr. Pierre Laporterie, of Hamburg.
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- Lettera enciclica dell' Abbate Fr. Perez, nella quale si fa palese la usurpazione plagiaria del suo stromento goniometrico triplindice fattogli da P. Eliseo della Concezione. Bologna, 1786. 4° _____
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mometer pieces.
- Commentarii de Rebus in Scientia Natu-
rali et Medicina geftis. Vol. XXVI.
Lipfiz, 1784, 1785. 8°
- Baron de Dietrich.
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F. R. S.
- The Author.
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P H I L O S O P H I C A L T R A N S A C T I O N S .

A.

- A**BRUZZO, Account of a Journey into the Province of, p. 365.
- Air, Atmospheric*, note respecting the weight of it, p. 127. A better conductor of heat than the Torricellian vacuum, p. 286. Experiments on the conducting power of moist air, p. 293. Experiments with air of different densities, p. 298.
- Air, Hepatic*, experiments on, p. 118. What it is, and its properties, *ibid.* General characters of, p. 127. The action of hepatic and other aerial fluids on each other, p. 130. The action of hepatic air, and acid, alkaline and inflammable liquors on each other, p. 137. The properties of water saturated with it, p. 141. The properties of alkaline liquors saturated with it, p. 143. Its constitution, p. 144. Of phosphoric hepatic air, p. 150.
- Alkaline Liquors*, their properties when impregnated with hepatic air, p. 143.
- Alum*, observations on the production of, p. 186.
- American Indians*, particulars relative to their nature and customs, p. 229. Enquiry into their supposed want of beard, *ibid.* Pluck out their beards, p. 230. Colonel Butler's opinion on this subject, p. 231. Captain Brant's opinion, p. 232.

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301. 3. from the bottom, for $36\frac{2}{3}$ read 40

302. 2. for $36\frac{2}{3}$ read 40

— 3. for 313 read 342

304. 6. for 313 read 342.

